SCIENCE.

FRIDAY, MAY 9, 1884.

COMMENT AND CRITICISM.

The German government has most commendably recognized the interest of the public in the reports made by the leader of the commission which has been studying the cholera in Egypt and India. The letters already so promptly published are, of course, merely notes of progress sent to the base of supplies; and no detailed and complete report can be expected at present. So far as the results have been made known, the work of the commission is full of promise. For the cholera, which, by the way, is only one of the subjects under investigation, a bacterium, apparently peculiar to the disease, has been found; and its cultivation has shown characteristics sufficiently marked to render its recognition easy. This comma-shaped bacillus has not, thus far, been found in connection with any other disease of the intestinal tract, although numerous examinations relative to this point have been made; and in cholera patients, it was only seen in association with the intestinal disturbance, but here invariably. It has, however, been met with in some sources of water-supply in India, in which the local infection may have originated.

It should not be forgotten that this work of Koch is no mere lucky guess. Bacteria were found by him in material sent to Berlin from India; but it was then impossible to decide how far putrefactive changes had produced them. The commission has now been able to examine a goodly number of fresh cases (fifty-two dead, and forty sick, from cholera), and thus to render the pathogenetic character of the bacillus exceedingly probable; and yet not a little remains to be done to complete the demonstration. Unfortunately, no inoculation experiments have thus far succeeded, owing to the remarkable insusceptibility of our household animals to cholera; and experiments on our own species are not permissible. It is also desirable to have more certainty as to the life-history of these bacilli, which may reach the victim as spores. The fact that they are chiefly found in the lower part of the small intestine suggests such a development, unless it be due to a temporary disablement of the bacillus as it passes the Scylla of gastric digestion, and the Charybdis of the bile inflow; the former being known to be dangerous, while the latter is inferentially so. Should Koch's conclusions prove to be correct (and, of course, corroboration by other and independent observers is desirable, and ought to be comparatively easy), then protection against cholera would seem to be a pretty simple task, even though its destruction at the fountain-head be impracticable. The germs do not appear to be very tenacious of life, so that an efficient prophylaxis can be readily exercised; and here a sound digestion becomes of primary importance for the individual. The season of intestinal disturbances is upon us, so that the work of the German commission can readily be supplemented in one direction in any of our hospitals.

Twelve years ago the thorough-going policy of the British admiralty in fitting out the Challenger expedition inspired us all with a hope that a new kind of governmental policy, in support of biological investigation, was being inaugurated. American as well as English naturalists have therefore been greatly disappointed, that, since the return of the Challenger, the British government has done practically nothing to forward marine research. The economists of the Manchester school are still in the ascendant; and the study of aquatic life is evidently to be left, like the hospitals, the asylums, the life-saving service, fish-culture, and the prediction of the weather, to private enterprise, either individually exerted or in combination in societies.

It was felt by many English men of science that a portion of the surplus of the late fisheries exhibition might appropriately be applied to the scientific investigation of the English seas, since this course would undoubtedly be very beneficial to the fishery interests of the nation. The very handsome sum remaining at the disposal of the directors has gone, however, almost entirely to build homes for the families of fishermen lost at sea. In deference to the vote of the British association for the advancement of science, in support of the plea of Professor Ray Lankester, a small sum is assigned to a 'Royal fisheries society,' yet to be organized, in whose future it is difficult to imagine any great benefit to result, either to science or to the fisheries.

Public opinion in Great Britain seems to demand the organization of a series of investigations similar to those which have for a number of years been carried on by our own fish-commission. At a meeting of fishermen in Peterhead, in January, a petition was forwarded for government aid for a scientific research into the habits of fish; and the representative fishery capitalists of Ireland are equally urgent. The meeting at the Royal society's rooms, a few weeks ago, for the organization of a 'Society for the biological investigation of the British coasts,' was evidently a part of the same movement. The endowment of fifty thousand dollars, which it is proposed to secure by private subscription, will doubtless be readily forthcoming; and we may safely predict for the new society the career of success which it deserves to have. Although not a direct outcome of the fisheries exhibition, it may fairly be considered one of its results.

The presentation of a petition, by a large number of Canadian naturalists, to the postmaster-general, requesting the government to "take into consideration the matter of a naturalists' exchange post for Canada, and for the other countries within the postal union," is a step which should meet the approval of naturalists in this country, by whom some organized attempt ought soon to be made to procure a modification of the existing regulations. As far as inland postage on specimens of natural history is concerned, no serious complaint can be urged against the postage charged, or the limit of weight allowed. The provision, however, that no written matter can be sent with the specimens, except at letter-rates, is a serious obstacle in many instances; for it frequently happens, that, as in case of marine plants mounted on paper, it is necessary to mark the locality and date on the paper at the time the specimen is collected. Without such written data, the specimens lose half their value. The rulings of the post-office department in Washington, with regard to written labels or notes giving the scientific name, locality, and date of collection, have been contradictory, and, as a matter of fact, naturalists are unable, except in an underhanded way, to send any but printed labels at the cheap rates; and, as every one knows, in by far the majority of exchanges labels must be written rather than printed. At the last meeting of the American association, a committee was appointed to consider the best way of presenting to the post-office department the claims of naturalists. It is said that the committee intend to report some plan of operation at the next meeting in Philadelphia.

With regard to foreign exchanges, of course no action can be taken without the action of the delegates of the postal union; and the Canadian naturalists desire to have the subject brought before the convention to be held in Lisbon next October. If we correctly understand the petition of the Canadian naturalists, they are now able to send packages not over eight ounces in weight, at sample merchandise rates, to countries in the postal union. If this is the case, they are much better off than we are in this country; for our post-office department has distinctly declared that no specimens of plants sent as botanical exchanges can be forwarded, except at letter-rates, no matter whether there is any writing

on the specimens or not. This is not the arbitrary ruling of any local office, but the written decision from headquarters in Washington. Such being the case, exchange of specimens with foreign countries is practically prohibited; and this seems all the more absurd, we may even say contemptible, when it is known that Christmas cards, and several other articles not classed in any way as samples, are allowed to be sent at sample-rates; furthermore, that from several foreign countries, packages of specimens are allowed to be sent to the United States at the cheap rate. Under the circumstances, it may, perhaps, be asked whether our Canadian friends are not going too far in asking that specimens not exceeding in weight four pounds, nor exceeding twenty-four inches in length by twelve inches in width or depth, be sent at the rate of one cent for four ounces. To be sure, such an arrangement seems to be eminently proper; and all naturalists should unite in bringing the measure before the Lisbon convention. In any event, the present embargo on scientific exchanges, whether caused by the illiberal interpretation of the rules of the postal union by our post-office, or by any ambiguity in the rules themselves, should be removed.

LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

Inertia.

As Mr. E. H. Hall (*Science*, vol. iii., No. 63, p. 482) referred to Maxwell, Thomson, and Tait, as *the* authorities in regard to the use of the word 'inertia,' it seems to me it would have been well for him to explain what Maxwell meant when, in reviewing Thomson and Tait's Natural philosophy, he said, —

"Again, at p. 222, the capacity of the student is called upon to accept the following statement:—

"'Matter has an innate power of resisting external influences, so that every body, as far as it can, remains at rest, or moves uniformly in a straight line.'

"Is it a fact that 'matter' has any power, either innate or acquired, of resisting external influences? Does not every force which acts on a body always produce exactly that change in the motion of the body by which its value, as a force, is reckoned? Is a cup of tea to be accused of having an innate power of resisting the sweetening influences of sugar, because it persistently refuses to turn sweet unless the sugar is actually put into it?" (*Nature*, vol. xx. p. 214).

Did Maxwell mean by these questions to deny the statement of Thomson and Tait?

S. T. MORELAND.

Lexington, Va., April 21.

The method of measuring the inertia of a body, proposed by Mr. Hall in No. 63 of *Science*, p. 483, is identical with a mode of measuring the mass of a body. Does he consider *inertia* as identical with *mass?* If not, wherein is the distinction? Whatever be the language describing it, or the ideas concerning it, Newton says it "differs nothing from the inactivity of the mass, but in our manner of conceiving it." Here *inertia* and *mass* are, by implication at least, not identical.

W.

April 23.

The recent article by Mr. Hall on 'inertia' is especially to be deprecated, because it may lead many to regard the ideas relating to it as in some sense indefinite. The source of the whole difficulty is that the word has been used in two perfectly legitimate senses, — one qualitative, and the other quantitative. In the qualitative sense, it simply implies the truth of Newton's first law of motion: in the quantitative sense, it is mass, and nothing else. This double use of the word has been fully recognized for a generation by all accurate scientific thinkers; and, on account of this ambiguity, all careful writers and teachers have practically long since abandoned it. Above all, it ought to appear in no text-book, just because it has a double sense.

This statement as to the usage of careful teachers is directly opposed to that of Mr. Hall, who mentions Thomson and Tait, and *quotes* Maxwell in support of the position which he occupies. As no teacher is clearer in his presentation of elementary ideas, nor more precise in his choice of words for conveying them, than Maxwell, either my statement or Mr. Hall's quotation demands revision. That the latter alternative is the proper one, I shall prove by quoting the whole of the passage of which Mr. Hall quotes only a portion of one sentence: —

"In a rude age, before the invention of means for overcoming friction, the weight of bodies formed the chief obstacle to setting them in motion. It was only after some progress had been made in the art of throwing missiles, and in the use of wheel-carriages and floating vessels, that men's minds became practically impressed with the idea of mass as distinguished from weight. Accordingly, while almost all the metaphysicians who discussed the qualities of matter assigned a prominent place to weight among the primary qualities, few or none of them perceived that the sole unalterable property of matter is its *mass*. At the revival of science, this property was expressed by the phrase, 'the inertia of matter;' but while the men of science understood by this term the tendency of the body to persevere in its state of motion (or rest), and considered it a measurable quantity, those philosophers who were unacquainted with science understood inertia in its literal sense as a quality, — mere want of activity, or laziness.

"Even to this day, those who are not practically familiar with the free motion of large masses, though they all admit the truth of dynamical principles, yet feel little repugnance in accepting the theory known as Boscovich's, — that substances are composed of a system of points, which are mere centres of force, attracting or repelling each other. It is probable that many qualities of bodies might be explained on this supposition; but no arrangement of centres of force, however complicated, could account for the fact that a body requires a certain force to produce in it a certain change of motion, which fact we express by saying that the body has a certain measurable mass. No part of this mass can be due to the existence of the supposed centres of force.

"I therefore recommend to the student that he should impress his mind with the idea of mass by a few experiments, such as setting in motion a grindstone, or a well-balanced wheel, and then endeavoring to stop it; twirling a long pole, etc., till he comes to associate a set of acts and sensations with scientific doctrines of dynamics, and he will never afterwards be in any danger of loose ideas on these subjects. He should also read Faraday's essay on 'mental inertia,' which will impress him with the proper metaphorical use of the phrase to express, not laziness, but habitude" (Maxwell's Theory of heat, pp. 85, 86).

It will be observed that Maxwell, instead of calling a certain property of matter *inertia*, and defining it

quantitatively in accordance with Mr. Hall's statement, is very careful to *avoid* using the term, putting it between quotation-marks in the only place where it enters. In short, in so far as a somewhat careful inspection of the book from which the above quotation is made, of his admirable tract on Matter and motion, and of his treatise on Magnetism and electricity, warrants me, I make the assertion that Maxwell never uses the word 'inertia' in a quantitative sense. I am confident that the word does not enter into the elementary book on mechanics in any sense.

In connection with the last paragraph from Maxwell, I quote a sentence from Mr. Hall's article (the italics are mine): "Maxwell suggests certain simple experiments which the student may perform in order to become thoroughly acquainted with that property of matter which *he calls inertia*."

Mr. Hall asserts, also, that Thomson and Tait use 'inertia' in the same sense which he recommends. As Maxwell's employment of the term is so different from what we should suppose from the article in question, I had the curiosity to look into the usage of the other authors named. I find the following passage, which forms § 216 of Thomson and Tait's Natural philosophy, vol. i., part i., new edition: —

"Matter has an innate power of resisting external influences, so that every body, so far as it can, remains at rest, or moves uniformly in a straight line."

"This, the *inertia* of matter, is proportional to the quantity of matter in the body; and it follows that some *cause* is requisite to disturb a body's uniformity of motion, or to change its direction from the natural rectilinear path."

This confused definition offers a marked contrast to the clear and extended definition of mass contained in sections which precede it. It is confused, because it admits of a wholly logical but erroneous conclusion. According to the definition, if we double the quantity of matter in a body, we double the inertia of the matter present, and thus quadruple the inertia of the body. This is absurd. What is meant, but not written, is, that the inertia of a *body* is proportional to the quantity of matter in the body. Let us consider this amended form, and write I and M for inertia and quantity of matter (or mass) respectively: then the assertion is, that

$$I = MX,$$

where X is a function of any thing or every thing except mass. Now, experience shows us that I, however defined, does not depend upon time, position, temperature, electrification, or, in short, upon any change in physical condition. We must conclude, then, that

$$X = C, \text{ a constant, and}$$
$$I = C\,M.$$

The numerical value of the constant will, in any case, depend upon the system of units selected for measuring I and M: therefore we may so select the system, that C becomes equal to unity, whence

$$I = M.$$

Here we see a case where an unnecessary, and, as it seems from a casual inspection of the following portions of the work, unused term is introduced as a survival from the period of 'the revival of science.' Of course, the passage does no harm to those who are competent to read the work which contains it: nevertheless, Maxwell would not have used it.

It is worth noting, that Mr. Hall, in the last paragraph of his article, finally gives a definition of mass as a quantitative definition of inertia. Of course, this is the only quantitative notion which can be attached to it.

A passage in the article under discussion reads, "Text-books too frequently say, in such a connection, that 'masses of matter receive motion gradually, and surrender it gradually,' or that 'it requires time to impart motion to a body as a whole,' — statements from which the student is in danger of getting the idea, if indeed he gets any idea, that the *time* is required in order to draw things taut within the body, and get its particles to acting upon each other, somewhat as it takes time and a succession of jerks to take up the slack of a freight-train while it is being started." Unlike its writer, I should recommend the sentences within quotation-marks to the special attention of the student, and emphasize the fact that *time* is required to transmit motion from one part of a body to another by the statement, that, in physics, this time is known as the measure of the velocity of propagation of a wave of disturbance. Finally, if I used the illustration of the freight-train (not a bad one in its way), I should be careful to explain to the student that the jerks are due only to the fact that the train is not mechanically homogeneous.

Obviously, the discussion of the term 'inertia' is not of the slightest scientific importance at this stage of scientific development; but it is of enormous pedagogical importance that loose ideas should not be taught. I have been prompted to the above remarks by appeals from some, who, supposing they had definite notions of elementary mechanics, had been led into confusion by Mr. Hall's statements.

C. S. HASTINGS.

Baltimore, April 24.

In *Science*, No. 63, Mr. E. H. Hall makes an attempt to clear away the mistiness which he seems to have discovered in the use of the word 'inertia.' No word in the English language deserves more sympathy than this. It has been knocked about so constantly that it must long ago have given up all idea of being able to 'persevere in a state of rest.' Lately there have been many indications of an intention to put it on the retired list in the near future, and for the present to assign it to such duties as it may be capable of performing without injury to itself or others. But Mr. Hall inconsiderately orders it to the front, and insists on endowing it with a real vitality, which, in the opinion of the writer, renders it capable of doing a good deal of harm.

Much of the confusion in the use of the word 'inertia' has originated in the various interpretations of Newton's first law. It is indeed curious to see how many different versions of this celebrated statement may be found in a half-hour's search.

Thomson and Tait, the restorers of Newton, say, 'Every body *continues* in a state of rest,' etc. To this form of statement it is difficult to object in any way. It is a simple statement of a fact, the denial of which "is in contradiction to the only systems of doctrine about space and time which the human mind has been able to form" (Clerk Maxwell). This version of the first law is identical with that of Tait in his Recent advances.

But another translator uses the word 'perseveres' instead of 'continues,' — the rendering so wisely chosen by Thomson and Tait; for 'to persevere' means, by common consent, something more than 'to continue.' Webster says, 'To persevere is to continue, *in spite of discouragements*,' etc. In an excellent and modern treatise on physics, the law is written, 'Every body tends to persevere,' etc., in which, evidently, 'persevere' is used in the generic sense of 'continue,' but in the ordinary sense, to 'tend to

persevere,' is not wholly satisfactory. In one edition of the Principia which lies before me, I find the statement that 'every body . . . endeavors to persevere in its present state,' etc. Here, certainly, we begin to see some trace of Mr. Hall's 'inertia;' and I should not be surprised to meet with the statement, in full harmony with his views, that 'every body tries to endeavor to persevere,' etc.

The beginner in physics is certainly liable to be confused in his endeavor to grasp this idea, — the idea of the mysterious resistance which Mr. Hall illustrates in his string-pulling; but his confusion will be vastly increased when he comes to grapple with the proposition, that "we must distinguish very carefully between inertia itself, a property of matter, and the resistance which matter can exert in virtue of that property," comparing it, as Mr. Hall does, with that property in virtue of which a man can exert force, and the force which he may be actually exerting at any time; and particularly when he is told that the resistance which he has considered is not the body's inertia, but is *merely the manifestation of that property!*

The unquestionable tendency of all this is to cause the student to attribute to the word 'inertia' some occult meaning. Most teachers of physics have encountered this condition of things, and have found some trouble in ridding their pupils of it.

Now, a brief analysis of Mr. Hall's own statements will unveil the mystery. If he had tied his string to the ghost of a fifty-pound ball, the resistance offered would have been nothing; at least, we may so affirm, in the present state of our knowledge in regard to ghosts. But the string was tied to a *mass*, and when he pulled it, he learned, that, in order to do work, work must be done. In short, the word 'inertia,' when properly used, is synonymous with 'mass;' and it is so used by nearly if not quite all the first authorities. There is, therefore, nothing mysterious about it, and, I may add, scarcely any reason for its use at all.

Mr. Hall mentions Maxwell, and Thomson and Tait, as apparently sustaining him in his view of the matter, quoting to a limited extent from the first.

Thomson and Tait, in their Natural philosophy, although not affirming that matter 'endeavors to persevere,' etc., do say that "matter has an innate power of resisting external influences, so that every body, as far as it can, remains at rest, or moves uniformly in a straight line." And this innate power is called 'the inertia of matter.' It is declared to be proportional to the quantity of matter in the body, and is afterward used as synonymous with mass.

This assertion of the existence of an 'innate power' bears the stamp of high authority, and one ought to question it with fear and trembling. But there is no evidence, that I have been able to find, that its authors believed in it themselves; that is, in the sense in which many people undoubtedly understand it. I have always regarded it as an unfortunate expression, which was likely to leave an impression which was never intended.

Professor Rankine, who was not careless in the use of terms, uses 'inertia' as meaning 'mass.'

Maxwell is universally admitted to have been a man of rare insight into the nature of things; and, as he is quoted by Mr. Hall, it may be interesting to see, as far as may be, what his position was on the point in question. His earliest public expression of opinion, as far as I know, was in his paper, 'On the properties of matter,' prepared at the age of seventeen years for Sir William Hamilton. This concludes as follows: "and the impossibility of a body changing its state of motion or rest without external force is called inertia." The next, as far as I know, is found in the Theory of heat, quoted by Mr. Hall. But in beginning the quotation where he does, Mr. Hall, unintentionally no doubt, does Maxwell an injustice. The sentence preceding that quoted is a most important and necessary part of the whole statement [quoted in full by C. S. Hastings, above].

It will be observed that this gives a perfectly definite meaning to the phrase 'measurable quantity,' and one quite different from that which might be inferred from Mr. Hall's fragmentary quotation.

Later came that remarkable 'little book on a great subject,' the Matter and motion; and it is a curious fact, and worthy of note, that the word 'inertia' does not occur in this book, not even in its compound form of 'moment of inertia.' It can hardly be believed that this omission was any other than intentional. His opinion of the 'innate power' may be learned from his review of Thomson and Tait's Natural philosophy [same quotation as given in first letter, above].

T. C. MENDENHALL.

In his article (*Science*, April 18), Dr. Hall writes as follows: "Elementary text-books usually speak of inertia as a mere *inability*, — the inability of a body to set itself in motion, or to stop itself when in motion. This is an old use of the term, but certainly not the best use."

Right here, I am constrained to believe, is Dr. Hall's fundamental error or misconception. He mistakes inertia for mass, and, strangely enough, laboring under this illusion, makes Maxwell use the word 'inertia' where in the text will be found the word 'mass.' For example: Dr. Hall goes on to say that "Maxwell suggests certain simple experiments which the student may perform in order to become acquainted with that property of matter which he calls inertia." Now, by reference to the article referred to, the reader will find Maxwell's words to be exactly as follows: "I therefore recommend to the student, that he should impress his mind with the idea of mass by a few experiments, such as setting in motion a grindstone, or a well-balanced wheel, and then endeavoring to stop it," etc.

Dr. Hall says, "We are driven to the conclusion that matter possesses a property in virtue of which it *offers resistance* to an agency which is setting it in motion." If Maxwell regarded inertia as an entity, 'a measurable quantity,' is it not remarkable that he did not even once, so far as I am able to find, use it in his incomparable work on Matter and motion?

If, as Dr. Hall is forced to conclude, "matter possesses a property in virtue of which it offers resistance," *why does it not resist?* Has a mass of matter, free to move, ever been known to 'stand still'? Certainly not: the whole science of dynamics will be overturned when such an instance occurs. The illustration given by Dr. Hall verifies *our* position. The fact that his heavy weight 'is left slightly swinging,' shows that a large mass will *not* resist the slightest force. Of course, the velocity generated will depend on the time of application. The whole thing is contained in the equation, $v = \frac{ft}{m}$. If m is large, and f small, t must be large to make v considerable. Thus, in the case cited, there is an attempt to make v considerable in a short time (t): therefore f must be large; and it is easily made larger than the string can bear, when, of course, it breaks.

In his second illustration, in which 'a weak thread'

is 'pulled gently and steadily,' is the reason that the fifty-pound weight acquires a greater velocity, because the weight resists less (if so, then resistance is less than itself), or because the time of application is greater?

In elementary works on physics, the word 'inertia' should be seldom used, lest the pupil acquire the impression that inertia is an entity. Most exact writers, foremost among whom is J. Clerk Maxwell, carefully avoid the use of the word. But if Dr. Hall's quasi-definition, given in the last paragraph of the article under discussion, is to be accepted, then must the word necessarily become one of constant use. It is a pity that Maxwell has not given us a definition of 'an inertia unit.' We shall be pleased to have Dr. Hall supply the desideratum.

A. P. GAGE.

In my article on 'Inertia' I was mainly concerned for the distinct recognition of a physical fact. My interest in the word 'inertia' was secondary. Professor Mendenhall and Mr. Gage appear to deny the reality of the 'resistance' of which I spoke in defining inertia. I said, "Matter possesses a property in virtue of which it offers resistance to an agency which *is setting* it in motion." Professor Mendenhall attempts to avoid the idea of a resistance in explaining the fact that force is required to set a body in motion, by speaking of the *work* done. The attempt seems to me entirely unsuccessful, unless he has some unusual definition of the word 'work.' According to Maxwell (Theory of heat, 4th ed., p. 87), 'work is done when resistance is overcome;' and, though he does not say that work is done *only* when resistance is overcome, no reader of Maxwell will deny that he meant that. This, by the way, is the only reply I need make to my critics' use of Maxwell's tea-and-sugar illustration; for certainly Maxwell considered setting a mass in motion to be doing work. With this I leave the question of physical fact, and come to that of the word or words used to denote that property which I have called 'inertia.'

In using the word 'inertia' as I did, I knew perfectly well that I assigned to it a meaning sometimes given to the word 'mass.' I knew that Maxwell, in the very passage of which I quoted a part, and of which Dr. Hastings has quoted the whole, used 'mass' as I have used 'inertia.' It was my belief, however, and it still is, that Maxwell, in that famous chapter, used 'mass' in two senses. He does use it as I have used 'inertia,' and in that case defines it as a '*property* of matter' (the italics are mine). Elsewhere in the same chapter he says, "What is really invariable is the *quantity of matter* in the body, or what is called in scientific language the *mass* of the body," etc. (the italics are mine).

As to Maxwell's use of the word 'inertia,' I was in error. I certainly spoke as if he gave undoubted sanction to the word in the sense in which I have used it. This I had no right to do, for he merely states what others have meant by this word. Any one, by reading the passage which Dr. Hastings has quoted from Maxwell, will see all the excuse I have to offer for my blunder.

Dr. Hastings admits that Thomson and Tait use the word 'inertia' to denote that property of matter for which I have used the same name; but he says that their statement is confused. This criticism is just; but it is irrelevant, unless Dr. Hastings means to imply that Thomson and Tait wrote 'inertia' where, in a clearer moment, they would have written 'mass.' Moreover, his commendation of their definition of the latter word might lead one to infer that Thomson and Tait use 'mass' as Maxwell does in the passage he has quoted. What, then, is their definition of 'mass'? It reads thus: '*The quantity of matter* in a body, or, as we now call it, the *mass* of a body," etc. (art. 208).

And now what is the practice of my critics in the use of the words 'inertia' and 'mass'? In the preface of Mr. Gage's Elements of physics, we read, "Dr. C. S. Hastings of Johns Hopkins university has read the larger portion in manuscript, and the remainder in proof-sheets." On p. 8 of this book I find, "By the *mass of a body* we understand *the quantity of matter in it*," and on p. 20, "The term *mass* is equivalent to the expression *quantity of matter*." Of course, the word 'mass' occurs in many other passages of the book; but I have discovered no case in which it appears to denote any thing but *quantity of matter*.

As to the use of 'inertia' in the same book, on p. 90 I find, "This inability is called *inertia*. Evidently the term ought never to be employed to denote a hindrance to motion or rest." But when we come to the subject of centrifugal force, p. 101, we read, "Centrifugal force has, in reality, no existence: the results that are commonly attributed to it are due entirely to the tendency of moving bodies to move in straight lines in consequence of their inertia."

Now, one of these results is the maintenance of the solar system. Why do not the planets, obeying the law of gravitation, fall into the sun? According to the teachings of this book, we must answer, "Simply because of their 'utter inability' to put themselves in motion, or to stop themselves, although this inability must never be understood as a 'hindrance to motion or rest.'" A little farther on in the book we read, it is true, that "to produce circular motion, the centripetal force must be increased . . . as the mass increases." 'Mass' enters here when the book speaks of numerical relations; but we see, that, when it attempts to *explain* 'centripetal force,' it appeals to 'inertia,' and says nothing whatever of 'mass.'

I think it not too much to claim that 'mass,' used to denote that property of matter which Thomson and Tait call 'inertia,' is comparatively rare, while one can hardly take up a book upon physics without finding 'mass' used in the sense of 'quantity of matter.' That an exceedingly intimate relation exists between inertia as I have defined it, and mass as commonly defined, I am well aware. Thomson and Tait's words are, "This, the *inertia* of matter, is proportional to the quantity of matter in the body." I should prefer to say, bodies of equal inertia (see the last paragraph of my article on 'Inertia') are assumed to contain equal quantities of matter. Quantity of matter, in this sense, is called 'mass.'

If it seems best to use 'mass' to denote also the property of matter which Maxwell undoubtedly does denote by it, let us so use it; and, by all means, let its double meaning be distinctly recognized in the elementary text-books. To me it seems far wiser, however, to use the two words, 'inertia' and 'mass,' substantially as Thomson and Tait use them, and to rigorously exclude from the text-books the comparatively useless 'inability' definition of inertia.

E. H. HALL.

Silk-culture in the colonies.

The term 'silk-balls' was doubtless employed at times to designate cocoons; but that is quite different from 'raw-silk' and 'raw-silk balls,' which, as we stated, might more appropriately apply to the twisted hanks of raw silk which are so doubled and

tied as to suggest such a designation. The choking or drying of the cocoons was in colonial days a part of silk-raising, and not of silk-reeling; and, while reeling-establishments may undertake to choke the cocoons brought in by the raisers in their immediate neighborhood or by agents, the marketing of fresh cocoons must necessarily be limited in time and distance. They cannot bear pressure without injury, and all baled cocoons must needs be choked. One is hardly justified in comparing the methods of colonial times with those in vogue to-day in France, where modern steam filatures and railroads have produced such profound modifications. We cannot see how choked cocoons, which have but one-third to one-fourth the weight of fresh cocoons, can be marketed at the same rates as the fresh cocoons. The term 'green' cocoons is often used in English as the equivalent of fresh cocoons; but, as quoted in the French markets of to-day, the word 'green' (*vert*) refers to those of a green or greenish color. Perhaps this may explain the puzzle. C. V. RILEY.

Thermometer exposure.

In No. 58 of *Science*, Professor Mendenhall calls attention to interesting differences of the minima temperatures on cold, still nights of the winter. I agree with him that a difference of exposure, and proximity to buildings, may explain a difference in reading; but it is impossible to explain by them alone the enormous difference noticed in Columbus (27.3° F.). There must have been, besides, one or another of the following conditions, probably both. When the conditions are favorable to radiation, and the night is still, the lowest strata of the air are mostly cooled by contact with the cold, upper surface of the ground; and more so if there is snow, and a so-called inversion of temperature is produced. The temperature rises from the lowest strata to a certain height. Examples of this can be found in the observations at Pulkova, near St. Petersburg. A thermometer placed at the height of seventy-eight feet was almost constantly higher than one at six feet above ground at eight P.M. In August, on clear days, the mean difference was 2.1° F., and once in September it was 5.2° F. In the months from December to March, when the ground is covered with snow, even at one P.M. the upper thermometer was higher than the lower; the mean difference on clear days of December and January at one P.M. amounting to 1.3° F., and once it amounted to 4.1° F.

The same results were obtained by experiments made at Kew, by direction of the meteorological office. The minima were lower at a height of twenty-one feet above ground than at a hundred and twenty feet; and on one occasion, at nine P.M., during a fog, the latter was higher by 10.8° F. than the former.

Now, most of the signal-service stations must have comparatively high minima, not only because they are mostly located in the interior of cities, but because the thermometers are often placed very high above the ground, at the level of the fifth or sixth story of city buildings. Probably the stations of the Ohio state service are placed lower.

Besides the height of thermometers above the ground, what I call the 'topographical conditions' are of importance. At an equal distance from the level of the ground, under conditions favorable to radiation, there will be much lower minima in valleys than on hills. This is caused by the descent of the coldest and heaviest air to the valley, and also by the fact that in a valley the air is in the vicinity of a greater surface of the ground. During the anti-cyclone of Dec. 19-30, 1879, the summit of Mont Verdun, near Lyons, France, had a mean temperature of —1.7° C.; and the Parc de la Tête d'Or, in the city, situated four hundred and fifty metres lower, a mean of —7.1° C. The mean minima differed by more than 12°C. Very likely the observations of the state service at Columbus were made on lower ground than those of the signal-service. Where anti-cyclones in winter are common in high latitudes, with the ground covered with snow, the mean temperatures of the winter months must be considerably colder in valleys than on the surrounding hills and mountain slopes, as the insolation during the day interferes but slightly, and not at all during some days at points beyond the polar circles, with the equilibrium of air strata obtained during the night.

This cold of the nights in valleys, subjecting plants to freezing on nights when those that grow on hills are spared, is well known. Perhaps it is less noticed in the United States, as there low temperatures are oftener accompanied by high winds than in Europe. The olive-cultivators in southern France, and the coffee-growers in the hilly districts of the province of San Paulo, southern Brazil, know this so well that they do not plant their trees in valleys, from fear of frosts. A. WOEIKOF.

St. Petersburg.

Dalmanites in the lower carboniferous rocks.

During a recent geological excursion near this city, one of our party, Mr. Henry Lane, found and pointed out to me a trilobite, which I extracted from the stone myself. The rock on which we were working was the upper part of the Cuyahoga shale of the Waverly group of Ohio, now universally, I believe, referred to the lower carboniferous system. The only genus hitherto reported from these rocks in America is Phillipsia, with the exception of two species of Proetus scarcely distinguishable from Phillipsia. The specimen in question, however, distinctly differs from both of these in the pygidium, the only part yet obtained. Instead of the evenly rounded and margined tail of those genera, it shows the flabellate and fimbriate form of Dalmanites. The occurrence of this genus or of this type of trilobite, so high in the geological series, is both surprising and 'uncanonical.'

E. W. CLAYPOLE.

Buchtel college, Akron, O., April 14.

'A curious optical phenomenon.'

Except in one curious point, 'F. J. S.'s' latest experiment (*Science*, No. 63, p. 475) obviously accords with my note (same page). Apparently, the virtual image is three feet *in front* of him, or nine feet from the wires, since the phantom rises when he bows; the slats are seventeen and two-thirds times wider apart than the wires, from centre to centre; and every fourth wire hides every third slat, while the next wire but one hides a slat-shadow. But how can thirty slats and their shadows thus give *twelve* dark phantom lines? With his telescope, 'F. J. S.' may find that two of them, least perfect, are where wires cross the *frame* of the blind.

Two words of mine, three lines from the bottom of the page, require correction. The size of the image is not 'very nearly' as described, but *exactly* so. If this image could become an actual screen, then *its* image, in turn, would be the farther screen; and any line through a wire-crossing in either of the three screens would meet the other two at points *quasi*-homologous to each other.

JAMES EDWARD OLIVER.

Cornell university, April 29.

A half-starved pig.

The following fact, though not unexampled, yet seems to me worth record. In the first week of September, 1883, on the farm of Mr. William Burr, in Medina county, O., the steam-thresher was at work; and, as usual, a large stack of straw was gradually accumulated. Two or three days afterwards Mr. Burr missed a fat sow weighing about three hundred pounds. After a long search and much inquiry, he came to the conclusion that she was lost in some unknown manner, and thought no more of her. About the 20th of March, 1884, in pulling down the remains of the straw-stack, the sow was found, thin as a deal board, but living. Her weight was a hundred and sixty pounds. She had been imprisoned for two hundred and five days, without water, and with only the straw for food. Treated with judgment, and fed slightly at first, she did well, and is now growing fat again. E. W. CLAYPOLE.

Buchtel college, Akron, O.

THE SCIENTIFIC METHOD IN HISTORICAL STUDY.

THE phrase 'science of history' suggests two very different things to different minds. To one kind of persons it means philosophical reflection and combination upon the course of human action in masses, in the purpose of finally discovering the laws by which such action has been governed, and then of applying these laws to prophesy about the future of the race. To these persons, Buckle is the ideal of a scientific historian. He alone, they fancy, has grasped the true principle of historic research, and truly shown the parallelism between the historical and the scientific methods. Just as the naturalist discovers his facts, and then combines them into laws, so the historian shall, it is said, proceed from single phenomena of human effort to the discovery of laws according to which all such human action has moved, and therefore must and will move. On the whole, perhaps, this is the view of historical science which prevails in the minds of most educated persons in America.

But there is another idea suggested by these words to those who have been accustomed to the thought and language of another school. These persons maintain that such effort is not historical work at all, but quite another science, dealing with the *results* of history. It is philosophy, with its general hypotheses and their more or less effectual support in discovered fact. All this should be called, not history, but the philosophy of history, just as there might be a philosophy of literature or of music, pursued successfully, perhaps with the best success, by men wholly untrained in either literature or music. Buckle and his kind, this school asserts, were not historians, but philosophers; and it claims for itself the more modest title. This we may, for convenience, call the modern German school, though it has its followers now widely spread in other lands. Not that Germans of our century have not cared to concern themselves with the wider problems of man's social and political destiny (nowhere, perhaps, have these problems received more thought than just in Germany), but this has remained the province of philosophers: and the men who have raised Germany to the leadership in modern historical research have, on the whole, kept themselves free from all speculation of the sort. To this school, then, the 'science of history' means the pursuit of historical knowledge according to scientific *method*. It concerns itself wholly with extracting from existing material the truth of the record. But to do this, it demands previously the most rigid examination and criticism of the material. For this examination, a wide and deep training in language, and in a general knowledge of the accepted historical tradition, is necessary; so that, while this German school is content to restrict itself within seemingly narrow limits, the man who would conform to all its demands finds a life-work before him, broad and severe enough to call forth all his intellectual energy. Its motto is found in the modest word of the elder Droysen, that the object of historical study is '*forschend zu verstehen*' ('to comprehend while investigating').

The study of history in America is in its infancy. It has remained until now an object of almost complete neglect in the programmes of collegiate as well as of secondary study. This neglect must have had a cause: we have no desire to force an issue between the two schools of historical study; but the fact cannot be overlooked, that, as long as American education remained under the influence of the early English tradition, history, as an item in education, was practically left out of sight. Men had, or professed, an enormous respect for it. One can read orations and lectures by the score, upon the usefulness of history as an element in the life of the present; but when it came to putting this usefulness into play, as a part of a scheme of education, giving to history a fair opportunity by the side of Greek, Latin, and mathematics, history had to give way. Men showed their respect for it by letting it alone. On the other hand, no sooner did the wave of German influence begin, about a dozen years ago, to beat with a violence that could not be disregarded, upon our shores, than the fortunes of historical teaching were

completely changed. Young men, returning from their study abroad, brought home with them this new principle, — to learn while, and by, investigating. All at once a new analogy to the study of nature began to be emphasized. The historian was to accompany the naturalist in his method of taking the thing to be studied in his hand, and applying the microscope to it; but this was to be done no longer with the ultimate purpose of deducing general laws of human progress, but simply of completing the record. Under this new impulse, history has now fairly begun to take its place by the side of other studies, as a subject demanding, in the widest sense of that term, a scientific treatment.

It would be a misfortune if either of the schools we have been examining should gain permanent and complete control over the other. Each has its claim to respect; but, for a long time to come, it seems clear that that view of the subject which has brought about so important, so decisive a change must remain the one to which our science must look for its support and its vindication.

These comments upon the condition of American instruction have been suggested by the appearance of two books, each in its way important for the future of the subject. 'Methods of teaching and studying history,' edited as the first volume of a proposed 'Pedagogical library,' by Dr. G. Stanley Hall, consists of an essay, occupying about half the volume, by Dr. G. Diesterweg, well known in Germany as the author of numerous pedagogical works, and of shorter contributions by professors in leading American colleges, together with an excellent short bibliography of general history by Prof. W. F. Allen of the University of Wisconsin. The importance of this book at the present time lies far more in its general purpose, and in its suggestion of a strong force behind it, than in any special excellence of its own. The treatise by Diesterweg is subject to the criticism, so often deserved by German writing, that it succeeds in obscuring the subject it tries to explain. The translation maintains all the obscurity of the original, and adds much of its own. The essays by American teachers were prepared, on what seems a wholly false principle, without any common understanding as to division of the field, and bear somewhat of the perfunctory character incident to most writing done at the demand of an editor. The various writers repeat each other; and the effect can hardly be to impress strongly upon the minds of teachers in the lower schools any effectual lessons for their own guidance. It is to be regretted that the German essay could not have been left out altogether, and replaced by something based upon a wider range of thought, and more pertinent to our American problem. If the American writers could have known each what the other was writing, the result would have been more harmonious, and the effect, as a whole, more decided. Yet one advantage has come from this defect: it has demonstrated how strong is the current which is now setting in the direction of what we may call, by a phrase which will cover many varieties of detail, 'teaching by topics.' There is complete agreement, among the writers, on this point, — that effectual teaching in history, as everywhere else, is that which rouses the student out of the dulness of a merely receptive condition, and puts him into the attitude of an original thinker. There would be a multitude of opinions as to the age at which this sort of work should begin, the exact form it should take, its proportion to the work of the memory, and so on. It is to be hoped that an opportunity will be offered for the further development of these points, — far more valuable for the teacher than a philosophical treatise in the cumbrous form of German metaphysical treatment.

Another point emphasized by some of the writers, and tacitly admitted by others, is the necessity for a steady progress of the student in the acquisition of a firm basis for his knowledge in space and time: chronology and geography, learned by a definite act of memory, according to one or another principle, must begin and accompany all study of history. This demand has called forth the second of the books referred to, — Ploetz's 'Auszug aus der geschichte,' translated and enlarged by Mr. William H. Tillinghast of the Harvard college library. This book was originally made for the use of students while engaged in detailed study, to furnish them with a substantial basis of general knowledge. It holds a middle position between a mere dictionary of dates and a connected narrative of general history. The work of translation has been done with something better than accuracy, — with a complete command of the original language, and a conscientious purpose to improve upon the material offered. The new volume is essentially a book for Americans. It will be welcomed by persons holding all shades of opinion as to historical methods, and ought to become a permanent factor in the new development through which the teaching of history is passing.

WINTERING IN THE ARCTIC.

A SHIP may winter in the ice under somewhat varied circumstances. She may be drifting in the pack during this time, unable to make a harbor, as in the cases of the Terror, Tegetthoff, Jeannette, Fox, and others (this may happen under two conditions, that is, whether liberated or not from the pack: these cases have been already noticed); or the ship may be frozen in, in the hummocky pack, but not subject to drift, as in the case of the Erebus and Terror, off King William's Land: or she may be safely ensconced in some good sheltered haven. In the first case, the most dangerous of all, it is seldom that any thing can be done but await events. A northward drift is a most perilous circumstance; and, although in the case of the Tegetthoff the crew managed to escape unscathed, it was only by a miraculous combination of favorable events. The disaster to the Jeannette and her unfortunate crew shows better what may usually be expected. It is this fact, to a great extent, that has led so many arctic expeditions to follow that continuity of shore-land which is swept by southward-trending currents, in preference to all others. Many arctic sailors of experience have even strongly contended that it is a matter to be at once considered, when a ship is thus probably circumstanced, if she should not be immediately abandoned before the northing gained would seriously compromise all hopes of escape. In a winter's drift it is impossible to properly 'bank' a vessel, as the incasing with snow-walls is generally termed, and it is consequently a severe labor to keep an equable temperature in the unprotected ship. In the case of the unfortunate Tegetthoff, "while in the berth close by the stove there was a temperature ranging between 100° F. and 131° F., in the other there was one which would have sufficed for the north pole itself. In the former a hippopotamus would have felt himself quite comfortable; and Orel, the unhappy occupant of it, was often compelled to rush on deck, when the ice-pressures alarmed us, experiencing, in passing from his berth to the deck, a difference of temperature amounting to 189° F." (Payer). The story of the Jeannette and the Terror also shows the miseries of unbanked vessels. In vessels properly 'banked,' however, no such variations of temperature need be encountered, even in the severest weather. The illustration (fig. 1) showing the Germania wintering in the ice is given to show an improperly 'banked' vessel, although well housed. Sketches (if they be accurate) of by far the greater majority of exploring-ships wintering in the ice show the same (and generally greater) lack of proper arrangements for keeping out the cold. A good contrasting picture is the one given (fig. 2) of the whalers wintering at Marble Island, in North Hudson's Bay; they being experienced icemen, and aided by Eskimo in the snow-construction. The sketch was taken by Mr. Klutschak of my party, while in the bay during the winter of 1878–79. I visited these ships that winter for a short while, and lived in one the next winter for no inconsiderable time; and, although the temperature outside was about the usual mean of arctic wintering-harbors, that inside was comfortable in all parts of the ships. To contrast with Payer's statement above, I would say, that, while the cabin showed about 80° or 85° F., the captain's room, separated from it by a door with lattice shutter, would seldom be over five or ten degrees lower; while in the 'houses' built over the ships it was generally a little below freezing, and very comfortable for persons who spent a proper time out of doors for exercise. This 'banking' is most conveniently done by Eskimo, when their services can be secured, as their superior ingenuity in snow-construction enables them to enclose the vessel in even several concentric snow-houses, thus securing the most equable temperature with

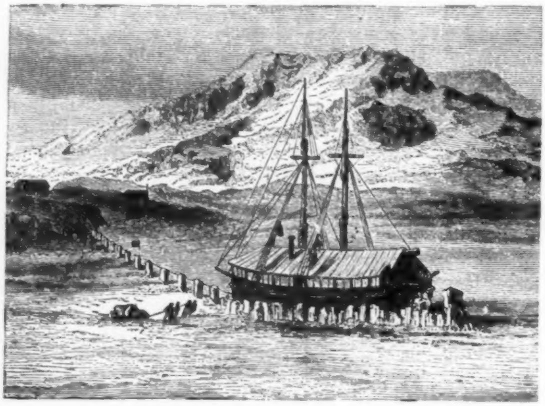


FIG. 1.—THE GERMANIA WINTERING IN THE ICE.

the least amount of material, which is quite a consideration when this monstrous mass has to be removed in the spring.

The drifting winter-beset ship has one advantage worth noting. If drifting towards warmer waters, as is generally the case in following the usual routes, she is almost certain of a safe and speedy release in the early spring months; and the constant state of alarm experienced by all ships' crews while in these involuntary journeys from ice-pressures, and threatenings of a general destruction of the ice-fields, has almost its compensation in the necessarily banished *ennui* and lonesomeness of the long polar night, with its accompanying evils of idleness and disease. Forced activity to overcome lonesomeness soon wearies, loses its effect, and becomes really a punishment, while that prompted by danger never loses its stimulating effect.

A vessel wintering in the ice, unable to secure a harbor, but not subject to drift, may be liable to much danger when the fields break up in the following summer; and this danger will generally be greater the farther she is from land, owing to her earlier liberation, probably long before the navigable season commences. In a vessel far from land much of the benefit derived from the voluntary exercise indulged in by the crew by short rambles and hunts is lost.

A vessel safely anchored in a good harbor is, of course, in the most favored condition of all. She may unbend her sails, lower her yards and topmasts, presenting a minimum of surface to the heavy arctic gales of that season of the year, while she is awaiting her freezing in, and which is especially necessary when the character of the bottom of the harbor is such that there is danger of dragging the anchors. Once frozen in securely, the anchor can be raised, the rudder cut out and unshipped, and all these, with masts, and yards, and spare stores, and provisions, may be placed on the shore conveniently by, and then room be made for the winter's entertainments, exercises, and studies. The very first thing a ship should do, after selecting her winter harbor, is to get ample provisions ashore, to be prepared for the loss of the ship by wreck or fire. This is always done by the whalers. A vessel is then 'housed in,' which is done by building a shed over the deck with lumber brought for that purpose. This house is generally about seven feet high, the lumber covered with canvas, this with a layer of moss or turf six inches to a foot thick, cut in the early fall before it has frozen, and dried as much as possible, and this layer of

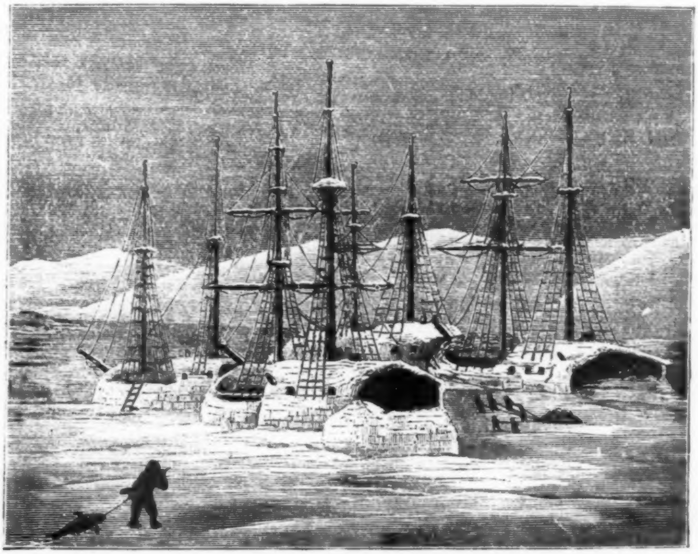


FIG. 2.—WHALERS AT MARBLE ISLAND.

turf again covered with from three to four feet of snow, which should be continuous with the snow-walls or snow-heaps placed along the sides of the ship. I give (fig. 3) my idea of a ship fixed for winter, shown in cross-section.

The 'house' is of inclined boards, covered with canvas, and again covered with dry turf. Inside it is lined with cheap canvas holding six or eight inches of 'mineral wool,' or other light cheap non-conductor; and this passes over just above the heads of the occupants. The snow-huts are shown by their cross-section of block-work, the inner air-space, *a*, being hermetically sealed, as far as it is possible with snow to do so. The second air-space, *c*, should be left open on warm days, that is, above $-10°$ F. to $-20°$. The house should run the whole length of the vessel, but be divided into two rooms for officers and men, and with only one door leading out, and that from the men's room. The stove in the cabin should have its draught flush with the level of the floor, and its stove-pipe within another of three or four inches more radius, and a propeller-blade ventilator run by clock-work in the latter to 'suck' air into the cabin. This will be the main source of ventilation, warming the air as it enters, and also protecting the ship from possible fire from the chimney. The draught will remove all foul air from the cabin-floor, and the companion-way will purify the lighter gases at the ceiling. Such a stovepipe as shown will obviate the great collection of frozen moisture around it, the descending cold air preventing the escaping warmth from melting contiguous snow and ice. The clock-work should be susceptible of regulation according to will, and run for at least twelve hours. At its exit from the outer dome of snow, the larger pipe should stop short of the smaller or inner, and be protected by a roof springing out from it, as shown in fig. 6. Light is secured by large thick blocks of ice placed in the sides of this 'house' at convenient intervals. If an 'igloo' dome be thrown over the vessel according to the proposed plan, the slabs of ice in it should directly face the double glass windows in the house proper.

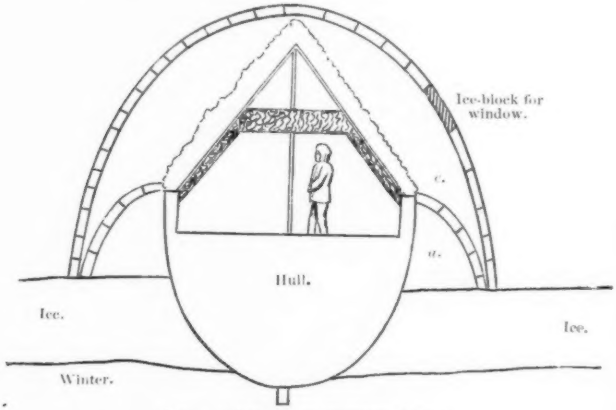


FIG. 3.—SECTION OF WINTERING SHIP.

If turf or canvas is not employed in the usual methods, the temperature of the house must be kept below freezing, or the continual melting of the snow, forming pools of ice on the ship's deck, will be disagreeable in the extreme. A housing solely of canvas, as has often been employed, prohibits the use of a thick layer of non-conducting snow or turf, and, except during a wind, it is but little better than no protection at all. The housing should extend the whole length of the ship if possible; but if cut short at the middle portion, a not unusual method to save lumber, the exposed deck should be treated to a covering of snow and turf similar to that placed on the house. Where moss or turf is not to be had, fine sand

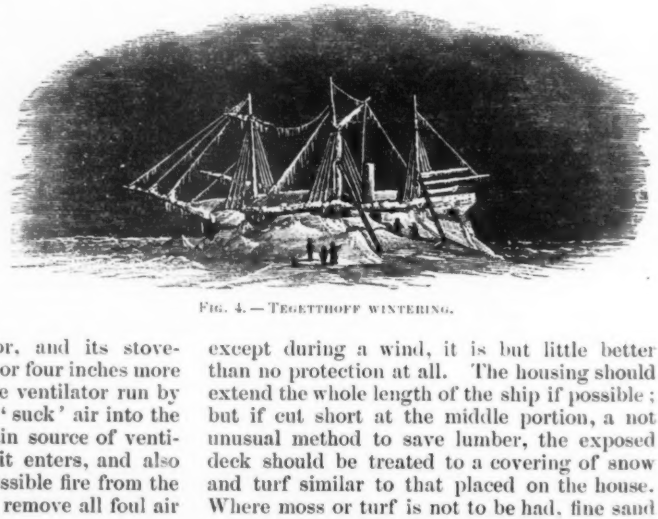


FIG. 4.—TEGETTHOFF WINTERING.

is not a bad substitute, but is much heavier, and can only be used on horizontal or slightly inclined surfaces.

The importance of securing a winter harbor near where Eskimo can visit the ships is not

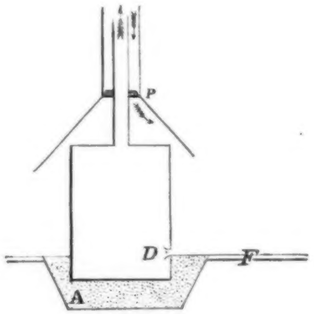


FIG. 5.

to be over-estimated. Besides their aid in snow-construction, the clothing procured from them is far superior to any that can be manufactured in civilization for withstanding the severe temperatures of those regions; their companionship does much to alleviate the lonesomeness of the winter's solitude, for they are generally a most cheerful, merry-hearted, and contented race; their services in procuring game from both land and water, to keep the crew in a healthy state,

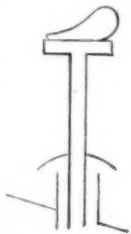


FIG. 6.

and especially to combat the scurvy, is apparent: while, in case of disaster, their humble abodes are always open to the shipwrecked sailor until there can be convenient times for retreat to reach more civilized succor, — a retreat in which the white men may be greatly aided by the native method of transportation.

A firehole being dug through the ice near by, which must be opened every morning and evening, and a snow-house (igloo) thrown over it (if natives are at hand to do the work) to protect it from drifting snow, our ship is ready to pass her arctic winter unmolested, until the coming summer opens a renewal of her labors.

Should the circumstances warrant a start early in the season, it will probably be necessary to cut a very long channel through from six to ten feet of ice, of sufficient dimensions to float the ship to the outer open water. The methods of cutting these channels vary. I show the one I have seen adopted, given in plan (fig. 7). The channel *B B B B* is always brought up *alongside* the ship, as shown; since, should she draw more water than the thickness of the ice, and the channel be brought up immediately behind her, the outgoing tide or a strong wind might break her loose, and sweep her out before it was intended she should move. The scarf-lines *c c*, *c c*, *c c*, formed in sawing, are sufficiently intelligible to be understood without an explanation; the ice-blocks, *A*, *A*, *A*, being allowed to float out along with the ebbing tide, a single person directing each one as fast as sawed off with a pike pole, to prevent its horizontal rotation, and consequent binding in the channel. Where the channel is long, and wind favorable, rough impromptu sails have been rigged on each ice-block to carry it out. If cutting very thin ice, as when cutting into harbor in the fall, these slabs can be shoved under the edge of the ice-channel. If the vessel delays her starting until after the solar rays have made considerable impression on the ice of the harbor, it will save much labor to remove the snow along the contemplated scarf-lines of the channel, and place there a covering of black seaweed, sand, dirt, or ashes, which will have cut deeply into the ice by the time the sawing is necessary. These layers, of course, should be very thin: otherwise they will protect the ice, instead of acting as ready conductors of the sun's heat. I noticed in the ice of Victoria Channel, off King William's Land, as late as the middle of July,

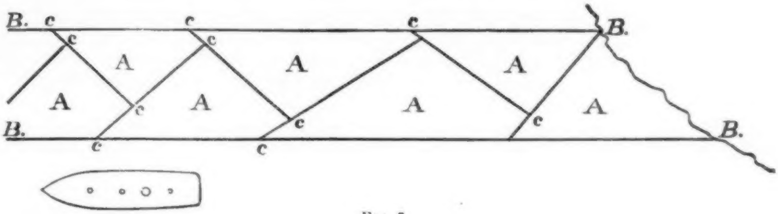


FIG. 7.

that a dark-colored kelp-stalk over twenty feet long had cut five feet into the solid ice a crevice not over an inch or two wider than the stalk, so that it was impossible to get it out.

The difficulty of sawing increases in a rapid ratio with the thickness of the floe; and, when its depth becomes so great as to allow a play of but a foot or two with the ice-saws, it becomes essentially impossible. Ice-saws, if very thick, impose severe labor on those operating them, by their great weight: if thin, they will warp and cramp in the thick ice, also creating severe labor. As all these contingencies cannot be foreseen, it is desirable to have a considerable assortment of these utensils, varying in length and weight. I think a description, however short, of ice-saws, is hardly needed, but will briefly speak of the ways I have seen them used. A 'one-man saw,' like the same named article in timber-sawing, can be used in ice up to four feet. Another foot, or even to work effectually in from three to four, requires two men, as shown in fig. 8; and it is evident, that, as the labor increases, the force at the bar can be increased, if the saw is only strong enough. As the floe gets thicker, the saw must be larger and have greater play, to work effectively; and this soon gets beyond the power of men and the reach in their arms, and a tackling is rigged, as shown in fig. 9, which can, I think, be understood without explanation. If the weight of the saw is not sufficient to pull it down, with the pushing assistance of the two men, its submerged end must be loaded with an anchor or anvil.

FIG. 8.

A small funnel-shaped harbor, with but few projections along its converging sides, may sometimes be relieved of all its ice at one time by a small amount of sawing along these serrated edges, and a happy combination of tide, wind, and good management. This is especially the case where the rise and fall of the tide exceeds the thickness of the ice, the consequent vertical oscillation of the ice keeping it broken up in hummocky masses along the shore-line.

The use of blasting-apparatus has, so far, been of but little use; still I think a series of small charges, fired electrically, giving rather a pushing than a splintering concussive effect, might be used advantageously in removing quite large masses of obstructing ice favorably situated. Blasting, I believe, would also be more efficacious in harbors not fed by fresh-water streams, as here the ice is more brittle, less tenacious and elastic, and consequently harder to remove by the percussive power of explosives.

A sailing-vessel can wait almost until she is liberated by the forces of nature, as this will probably be the earliest date that she can use her peculiar motive power effectively.

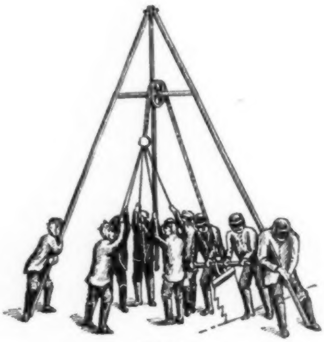


FIG. 9.

Even a good harbor may have its disadvantages for a ship, if she has entered it during an exceptionally open season; and, unless this recurs within the time for which she is provisioned, she must be abandoned to save the lives of the crew. McClure's Investigator in the Bay of Mercy, in 1854, is an example of such necessary abandonment.

The use of balloons to make slight ascents, — they being made fast to the ship, — to enable the ice-master to obtain a more comprehensive view of the state of the ice, has never yet been experimented with, though by many recommended, and consequently can be neither rejected nor accepted as an auxiliary in this sort of cruising. Certain it is, however, that nothing is more deceitful at times than ice-packs or ice-drifts at a distance; the most invulnerable-looking, upon a closer examination, proving to be the most disjointed, and the reverse. No arctic ship, of course, will be without her 'crow's-nest' of the whalers, — an elevated 'lookout' on the foremast, with good protection from inclement weather, for her ice-master.

The advantage in having two ships over one is apparent. It proved the salvation of Parry on his third journey, and other instances are not wanting. The benefit of two crews to cut in or out of harbor, and in other work where it is the same for one as a dozen vessels, is not to be overlooked.

In general, near the magnetic pole, the ship's compass is more or less worthless, its sluggish oscillations being easily overcome by the most insignificant local attraction, which it is almost impossible to avoid on shipboard. The farther removed from this great centre of magnetic force, necessarily the more reliance can be placed on the needle. The simple plan of rudely determining the points of the compass by a watch or chronometer regulated to mean time, conjoined with the motion of the sun in azimuth, will be sufficient in a land where the sun is shining throughout the day, and especially when the navigation depends rather on the bearings of the 'leads' and ice-barriers than any determinate direction. The fact that a vessel should follow a continuity of land, if possible, lessens the importance of the compass while capes and head-lands can be kept in view.

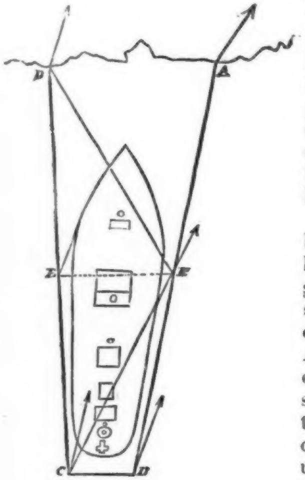


FIG. 10.

The 'ice-blink' is a well-known yellow glare that seems to hang over pack-ice. Any channels of natural sky seen through the glare indicate open water under them; and this is of use in approaching ice. In fact, the 'ice-blink' is more marked when at a distance from a pack in open water than when in one pack viewing another at the same distance.

Having explained ice-sawing, and hinted at a 'dock,' I will briefly describe an artificial one, and take as a typical example the case of the Alert, docked in the ice, Aug. 12, 1875, in Smith Sound. A plan of the dock is given in fig. 10. It was cut in about four hours, and could have been done, says the commander, in half the time, with a better organization and more experienced ice-cutting crew. It is a necessary operation to prevent being crushed between two bodies of ice, when the time will allow it, and also when a natural dock, formed of irregular blocks or floes, is liable to be obliterated by the increasing pressure eroding the fragile edges of the blocks. In this latter case a dock cut into the solid side of the largest block or floe would probably be a safe haven. The use of steam has rendered docks much less necessary than formerly, as the time occupied in cutting one will allow almost any steamer to escape any average danger.

Although, from this rather long list of probable arctic accidents to which a ship is exposed, escape would seem rare, yet, after all, it is wonderful to notice the small number of craft actually lost in this dangerous species of navigation, in proportion to the whole number engaged. Only those that are lost under tragic circumstances being brought before the public, they are generally supposed to be the greater majority of those thus employed.

FRED'K SCHWATKA,
Lieut. U. S. army.

BESTOWAL OF THE GRAND HONORARY WALKER PRIZE ON PROFESSOR JAMES HALL.

SOME years ago Dr. William J. Walker gave to the Boston society of natural history a prize-fund, from which, in accordance with the terms of the gift, annual awards are made to successful competitors who have written essays on assigned questions. But, besides these annual awards, a grand honorary prize was provided for, to be given every five years, and which the society was to grant, on recommendation of a special committee, "for such investigation or discovery as may seem to deserve it, provided such investigation or discovery shall have been made known or published in the United States at least one year previous to the time of award."

The society, in previous years, has awarded this honorary prize, amounting to five hundred or a thousand dollars, at the option of the society, to Mr. Alexander Agassiz and to Professor Joseph Leidy. This year the committee, after due consideration of the subject, has unanimously concluded to recommend for this prize, Professor James Hall of Albany; and the award of the highest sum was accordingly made by the society, at its meeting of May 7.

As the founder would appear to have contemplated some particular or integral 'investigation or discovery,' "we need not," says the committee, "take into account Professor Hall's numerous works or publications upon North-American geology and paleontology for the last forty years and more (comprised in about twenty-six volumes or parts of volumes, and in over two hundred articles or papers, reports, etc.), except as they relate to a special line of investigation, which Professor Hall early made his own, in which he has long been eminent, and which he may be said to have essentially completed, although a considerable portion of the results, which have been from

time to time 'made known' to the scientific world, are not yet published *in extenso*, with the illustrations prepared for the purpose.

"It is, then, specifically for Professor Hall's investigations in North-American paleontology, notably the paleontology of the state of New York and the regions adjacent, and of the earlier geological formations, that the committee suggests this award. In this field Professor Hall holds a position like that which has been so long occupied in Europe by Mr. Barrande. If his actual publications are as yet less extensive than those which have made the name of Barrande illustrious, this has not been from the lack of material, still less from lack of industry and scientific acumen on Professor Hall's part, but because he has not enjoyed the advantages of independent fortune and munificent patronage. Giving due credit to the state of New York for what it has done to further the publication of researches in its service, it still appears that his prolonged labors have been carried on under many discouragements and with insufficient means. It is understood, however, that deficiencies in this respect are about to be remedied; and it is hoped that this veteran paleontologist may have the satisfaction of superintending the full publication and proper illustration of his completed investigations.

"In recognition of the great value of the scientific work to which Professor Hall's life has been so untiringly and successfully devoted, in encouragement of his closing labors, and in testimony of the society's high appreciation of these services to science, your committee would recommend that the maximum of the prize be awarded upon this occasion."

PORTRAIT OF PROFESSOR JAMES HALL OF ALBANY.[1]

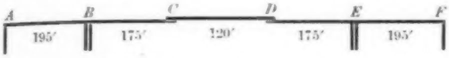
[1] From a crayon drawing, after a photograph taken for *Science*, April 17, 1884, by T. W. Smillie, photographer of the U. S. national museum.

THE CANTILEVER-BRIDGE AT NIAGARA FALLS.

THE new bridge across the Niagara River, built to connect the Canada southern railway with the New-York central and Hudson-River railroad, and opened for traffic in the early part of the present year, has been widely noticed in the newspapers, and referred to as a marked advance in engineering. Quite a general interest in regard to it has therefore been aroused by the apparent novelty of the design, and the rapidity of construction. As the railway suspension-bridge is below, and within some three hundred feet of, the cantilever-bridge, the contrast between them is forced upon every observer. While the cost of the two bridges, aside from the approaches, was probably very nearly the same, the suspension-bridge required three years for its construction, and will carry one train and such load as may come upon the lower roadway; the cantilever-bridge was erected in seven months and a half from the beginning of the work, and is designed to carry a freight-train on each of its two tracks at the same time, each headed by two seventy-six ton engines, and crossing without slackening speed. The ability to accommodate a greater traffic, and the rapidity of construction, may justly be ascribed to the advances made in American types of iron bridges.

One of the first questions asked concerns the meaning of the term 'cantilever.' It signifies, as an architectural term, 'a bracket, or projecting member, to support a load, such as a cornice or balcony.' The illustration accompanying this article gives a very good view of the structure as a whole; and the action of the cantilevers, as well as the several members, can be understood from the following diagram.

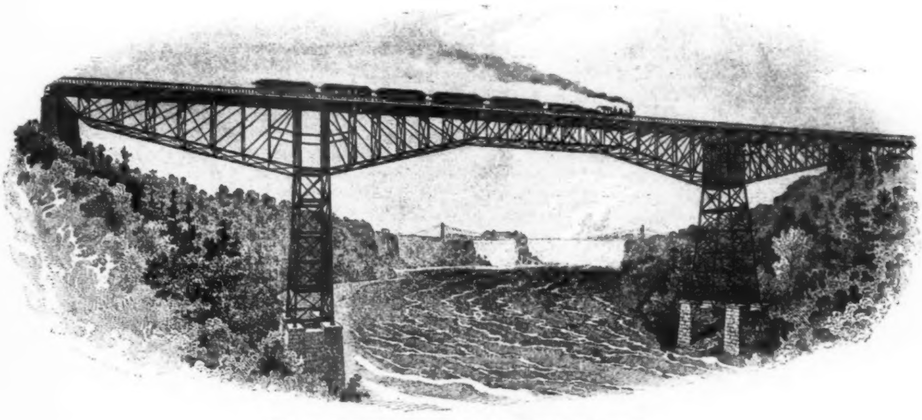
The shore abutments are at *A* and *F*, nine hundred and ten feet apart. The piers *B* and *E*, having a width on top of twenty-five feet, support two trusses,



A C and *D F*, the lengths of whose arms are marked below them: on their outer ends rests the independent truss *C D*, one hundred and twenty feet in length. The parts *B C* and *D E* are the cantilevers, carrying the truss *C D*, and projecting from the piers *B* and *E* as a bracket from the face of the wall. The ends *A* and *F* are prevented from rising, under any load between *B* and *E* which may not be balanced by the excess of weight in *A B* and *E F*, by anchoring bolts at *A* and *F*, extending to iron beams placed beneath the shore abutments. These abutments weigh one thousand tons each: the maximum lifting-force to which either one will be subjected is three hundred and forty tons. The expansion and contraction from changes of temperature are provided for between *B* and *E* by joints at *C* and *D* which allow longitudinal motion, and at *A* and *F* by pendulum links which permit a similar movement.

A detailed statement of the rate of progress in construction and erection will show quite clearly the advance made in late years in the art of bridge-building, and the ease with which structures of the American type, jointed at intersections and connected by pins, can be put together. The contract for the bridge was signed on April 11, 1883; and a clause was included by which the builders would forfeit five hundred dollars per day for all time required to finish the structure after Dec. 1. Ground was broken for the foundations of the towers, April 15. Laying of the concrete foundation, eight feet thick, began on June 6; and of the masonry piers, thirty-eight feet high and twelve feet square on top, June 26. The masonry on the American side was finished Aug. 20, and on the Canadian side Sept. 3. The two towers or steel piers, each of which has four legs, sixty feet by thirty feet apart at the base, twenty-eight feet by twenty-five feet apart at the top, one hundred and thirty-two feet high from the top of the masonry to the bottom of the truss, and thoroughly braced in all directions, were begun Aug. 29, and completed Sept. 18.

Scaffolding or false-works for the support of the portions *A B* and *E F* having been put up, these shore-arms were built upon it in the usual manner of bridge-erection, and were finished in time to begin construction of the river-arms on Nov. 1. This portion of the bridge was built out, piece by piece, triangle by triangle, from the piers, with no other outside support than a travelling framework above and projecting forward from the bridge itself: this traveller carried a suspended platform to insure the safety of the workmen. The sections from the two shores were built out and joined Nov. 21, without serious accident or delay. The track was down, ready for a train, in seven months and a half from the beginning of the work, and with eight days to spare on the contract time.



THE CANTILEVER-BRIDGE OVER THE NIAGARA RIVER.

The bridge has two trusses, twenty-eight feet apart, fifty-six feet deep over the towers, twenty-one feet deep at the shore-ends, and twenty-six feet deep at the mid-span. Ample wind-bracing is provided. The material used in the towers and heavy compression members is open-hearth steel: most of the other members are of wrought-iron. One admirable point in the design of the engineer, Mr. C. C. Schneider, and in the way in which it was executed by the builders, the Central bridge-works of Buffalo, N.Y., was the fact that no piece, while the bridge was in process of erection, was subjected to a strain greater than, or different from, what it must undergo in the completed structure. At the formal test and open-

ing, Dec. 20, the bridge was traversed by two trains, advancing side by side from one end, and composed of twenty locomotives, and enough cars loaded with gravel to together cover both tracks completely. The independent span *C D* was occupied entirely by engines when the bridge was fully loaded. The deflection of the point *C* under the test was between six and seven inches, being an aggregate arising from the yielding of *A B*, the compression of the tower, and the deflection of *B C* itself. On the removal of this load, of double the amount which will probably ever be imposed upon the structure, the bridge completely recovered itself.

The application of the cantilever in bridge-building may be seen in several other instances in this country. Sometimes it has been used to diminish the opening to be spanned by a single truss, and more frequently it has been introduced to facilitate the erection of a bridge in places where temporary supports in mid-channel could be obtained only with great difficulty and expense. A wagon-bridge at Fort Snelling, Minn., furnishes an example of the former class; but the cantilevers are reduced to simple triangular brackets, projecting some thirty or thirty-five feet beyond the faces of two adjacent piers, and reducing the span to two hundred feet. The Cincinnati southern railway bridge, over the Kentucky River, has three spans of three hundred and seventy-five feet each. As the gorge which this structure crosses is two hundred and seventy-five feet deep, and ordinary false-works were out of the question, the spans were built out from each cliff as projecting trusses, anchored back to the rock. By the aid of one temporary timber tower on each side, and the iron piers, the bridge was thus joined in the middle. The lower chord connections were then severed at three hundred feet from each bank, leaving the middle span with a cantilever of seventy-five feet projecting from each of its ends. Here the introduction of hinges obviated the changes of strain which would otherwise be caused by the effect on the tall iron piers of changes of temperature. The Minnehaha bridge, across the Mississippi River, between St. Paul and Minneapolis, has three spans, and was erected like the Niagara bridge, — the two shore-arms on false-works, and the middle span as two cantilevers, which are connected by a hinged joint in mid-river, without any independent span.

A design for the Frazer-River bridge on the Canadian Pacific railway, by Mr. Schneider, although not yet erected, antedates the Niagara bridge, and is like it, only on a smaller scale. The design for the Blackwell's Island bridge, across the East River, New York, which was awarded the first prize in 1876, introduced cantilevers and an independent span. A similar type of bridge is in progress at St. John, N.B.; one is proposed for the new Harlem-River bridge, New York; and the great bridge for crossing the Frith of Forth, now under construction, is a bold design of this type, having two openings of seventeen hundred feet each. Others might be mentioned if space permitted.

CHAS. E. GREENE.

THE CHOLERA BACILLUS.[1]

THE question, which, in my last report of Jan. 7, was left unanswered, — whether the bacilli found in the intestines affected with cholera are parasites due to cholera alone, — may be looked upon as answered.

It was at first exceedingly difficult, on account of the varying conditions under which the pathological changes took place in intestines affected by cholera, and on account of the great number of bacteria constantly present in them, to find out the bacillus proper to the disease. In most cases death occurred, not at the height of the cholera process, but during the period of reaction immediately following, in which such important changes take place in the condition of the intestines and their contents, that it is impossible, from such cases alone, to gain a clear conception of the cholera process. Only when one has had an opportunity to dissect a number of uncomplicated cases, and to compare with them the conditions exhibited in persons when first attacked, is it possible to gain a correct insight into the pathological conditions of cholera. On this account it was always kept in view, to use the greatest caution in accepting any theory as to the connection of the bacterial condition and the cholera, or as to causal connection of the bacteria with cholera, till the full proof might be obtained.

In the last report, I could already state that the peculiarities of the cholera bacteria were so well determined that they could safely be distinguished from others. Of these characteristics, the following are the most striking: the bacilli are not perfectly straight, like other bacilli, but slightly curved, like a comma. The bending may go so far that they take the form of a half-circle. In the pure cultivation from these bent rods often arise s-formed figures, and more or less long, slightly wavy lines, of which the first are made up of two, and the last of a large number, of the cholera bacilli, which, by continued increase, have remained connected. They possess powers of locomotion, which can best be seen, and in most marked degree, in a drop of cultivation-liquid suspended on a cover-glass: in such a preparation, one sees the bacilli moving with the greatest velocity in all directions through the field.

Especially characteristic is their action when cultivated in gelatine, in which they form colorless colonies, which at first are closed, and appear as if they consisted of very brilliant little glass particles. Gradually these colonies liquefy the gelatine, and spread out to a considerable extent. In gelatine cultivation they are, therefore, through this remarkable appearance, very surely distinguished from other bacteria colonies, and can easily be isolated from them. Moreover, they can pretty surely be distinguished by cultivation in hollow slides, as they always go to the edge of the drop, and in that position can be recognized by their peculiar movements,

[1] Sixth report of Dr. KOCH of the German cholera commission, dated Calcutta, Feb. 2, 1884. Translated from the *Berliner klinische wochenschrift* for March 31. An abstract of the seventh report will be found in the Notes and news.

and, after application of aniline solution, by their comma form.

As yet, twenty-two cholera bodies and seventeen cholera patients have been subjects of investigation. All these cases were studied for the presence of the specific bacteria, as well with gelatine cultivation as also in microscopical preparations, for the most part through cultivation in hollow slides; and, without exception, the comma-shaped bacilli were found. This result, together with that obtained in Egypt, justifies the statement that this kind of bacterium is always to be found in the cholera intestine.

For corroboration, moreover, investigations were carried on in the same way on twenty-eight other bodies (of which eleven had died from dysentery); the evacuations of one case each of simple diarrhoea, dysentery, and of a convalescent from cholera; then from several well people, as well as on animals dead from ulcer in the intestine, and pneumonia; finally, also with putrid masses of impure water (various samples from city sewage, water from very impure swamps, swamp scum, and impure river-water): but in not a single instance did it happen, either in stomach or bowels of the bodies of man or beast, in evacuations, or in fluids rich in bacteria, that the cholera bacteria was found. As by arsenic-poisoning a sickness very similar to cholera can be induced, an animal was killed by arsenic, and, after death, the digestive organs examined for the comma bacillus; but with a negative result.

From these results the further conclusion may be drawn, that the comma bacillus is peculiar to cholera.

As to the connection of this bacillus with cholera, it was carefully stated in the last report, that there may be two views: 1°, that the condition of the organs of a person sick with cholera is such that this peculiar bacillus prospers; 2°, that the bacillus is the cause of the cholera, and that only when it makes its way into the bowels of man can the sickness take place. The first supposition is not allowable from the following grounds: it would be necessary to grant, that, when a man is taken sick with the cholera, this bacillus was already present in his organs, as shown by its universal presence in the considerable cases investigated in Egypt and India, two widely separated lands. This could not be the case, however; since, as has already been pointed out, the comma-shaped bacillus is never found, except in a case of cholera.

Even in cases of bowel affection, such as dysentery and bowel catarrh, to which cholera very often supervenes, they fail. It is also to be considered, that, if this bacterium were always present in man, it would surely have been observed on some occasion; which has not been the case.

As the increase of this bacterium cannot be brought about in the bowels by cholera, the second supposition, that it is the cause of cholera, only remains. That this is, in fact, the case, is shown unquestionably by other facts, and especially by its behavior during the progress of the disease. Its presence is restricted to the organ in which the disease is, — the bowels. In vomit, they have, as yet, only been noticed in two cases; and in both, the appearance and alkaline reaction of the vomited fluids showed that the contents of the bowels, and with these the bacteria, had got into the stomach. In the bowels their history is as follows: in the first evacuations of the patient after the attack, as long as they have any form, very few cholera bacilli are present; the watery, odorless evacuations which follow, on the contrary, contain the bacilli in great numbers; while, at the same time, all other forms disappear almost entirely, so that, at this stage, the cholera bacilli are cultivated practically alone in the bowels. So soon as the cholera attack lessens, and the evacuations are again fecal, the comma bacteria disappear gradually, and are, after the convalescence, no longer to be found. The same is found to hold in cholera subjects. In the stomach no cholera bacilli were found. The bowels varied, according as death had occurred during the cholera attack or after it. In the freshest cases, the bowels showed a clear, red color; the inner lining of the intestines was still free from submucous extravasation; and the contents consisted of a colorless, odorless liquid: the cholera bacilli were present in enormous masses, and nearly pure. Their distribution corresponded exactly with the degree and spread of the inflammation of the lining-membrane, the bacilli being generally not so thick in the upper intestine, but increasing toward the lower end of the smaller intestine. When, however, death has taken place later, the intestines show signs of an important reaction. The lining is dark red in the lower part of the smaller intestine, impregnated with extravasations of blood, and often dead on the outermost layers. The contents of the bowels are, in such cases, more or less blood-colored, and, in consequence of the re-appearance of the bacteria of decomposition, putrid and fetid. The cholera bacteria at this stage begin to disappear, but continue still to be present for some time in the solitary glands and in their vicinity, — a circumstance which first called attention to the presence of this peculiar bacterium in the bowels of the Egyptian cholera subjects. They entirely fail in such cases, only when the patient has lived through the cholera, and dies from the after-weakness.

The cholera bacteria act exactly as other pathological bacteria. They occur only in their peculiar disease; their first appearance is when the illness begins; they increase in number with the severity of the attack, and gradually disappear as the illness wanes. They are found where the trouble exists; and their number, at the height of the disease, is so great, that their injurious effect on the lining of the intestines is explained.

It might well be wished that it were possible, with these bacteria, to engender in animals a disease akin to cholera, that their causal relation to the sickness might be made the more clear. This has, as yet, not been done: whether it will ever be done may well be questioned, as animals do not appear to be subject to cholera infection. If any kind of animal could take the cholera, then such a case would have been observed in Bengal, where, during the whole year, and

over the whole country, cholera infection is spread. But all reported cases have, as yet, failed of corroboration. Nevertheless, the evidence of the facts produced cannot be weakened by the failure of the experiments on animals. With other infectious diseases, the same observation has been made; for example, in the case of typhoid fever and leprosy, — two diseases for which specific bacteria are known, without, as yet, its being possible to communicate them to animals; and yet the manner of the occurrence of the bacteria in these diseases is such, that, without doubt, they must be looked upon as the cause of the disease. The same holds true for the cholera bacteria. Moreover, the further study of the cholera bacteria has made known many of their peculiarities, which all agree with that which is known of cholera etiology, as well as further evidence of the correctness of the assumption of the bacteria as the cause of the disease.

In this connection it is well to state the often observed fact, that in the linen of cholera patients the bacteria increase in a most remarkable manner, when the clothes have been soiled with the evacuations, and then, for twenty-four hours, have been kept in a moist condition. This explains the known fact, that the people having to do with such affected linen are often attacked. On account of this, further experiments were instituted; and cholera evacuations, or the contents of the intestines of the dead, were spread on cotton, on paper, and especially on the damp surface of the ground. After twenty-four hours, the thin sheet of slime invariably changed into a thick mass of cholera bacilli.

Another peculiarity of the cholera bacteria is, that they die, upon drying, much more quickly than most others. Commonly all life is extinct after three hours' drying.

It has also been noticed that their development only takes place well in substances having an alkaline reaction. A very small amount of free acid, which would have little or no effect on other bacteria, puts a marked check on their growth.

In a healthy stomach they are destroyed, which is shown by the fact that neither in the stomach nor the intestines of animals which had been constantly fed on cholera bacilli, and then killed, were any found. This last peculiarity, together with the impossibility of their withstanding drying, gives an explanation of the every-day observation, that infection so seldom occurs from constant intercourse with cholera patients. Evidently, that the bacilli may be in condition to pass the stomach, and bring about the cholera in the intestines, peculiar conditions must be present. Perhaps, when the digestion is imperfect, the bacilli might be able to pass the stomach; and the fact observed in all cholera epidemics and in India, that those suffering from indigestion are especially subject to cholera, may bear out this view. Perhaps a peculiar condition, analogous to the period of inaction of other bacteria, would enable them to pass the stomach uninjured.

It is, on the whole, not probable that this change in the production of inactive spores exists; then such spores, by observation, are known to remain months, or even years, capable of life, while the cholera poison remains active not longer than from three to four weeks. Nevertheless, it is conceivable that some other form of inactivity exists, in which the bacilli can retain their life in a dry state some weeks, and in which they withstand the destroying influence of the stomach.

The conversion into such a condition would correspond with that which Pettenkofer has designated as ripening of the 'cholera-infection material.' As yet, such an inactivity of cholera bacilli has not been discovered.

THE EXPLORING VOYAGE OF THE CHALLENGER.

(*First notice.*)

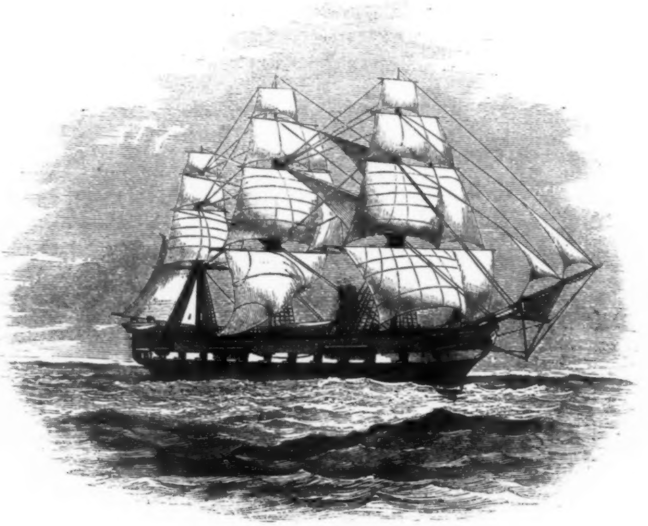
THE Challenger was a British man-of-war, a corvette of twenty-three hundred tons, equipped at the public expense with every appliance for the scientific study of the sea and of marine life, and carrying a faculty of six civilian specialists chosen by the Royal society, in addition to a staff of naval officers selected with reference to their scientific attainments.

This floating laboratory was sent out in 1872 upon a voyage of discovery around the world, and, during an absence of three years and a half, visited every accessible sea and ocean, traversing a distance of nearly sixty-nine thousand miles. Three hundred and sixty-two observing-stations were established at sea, and over five hundred deep-sea soundings made, — a wonderful record of industry, when it is remembered how many weeks were necessarily spent at coaling-stations, and when we take into account the fact that the present methods of rapid work by means of thin-wire dredge-ropes had not then come into use, and that a dredge-haul from a depth of two thousand to twenty-five hundred fathoms, which the Blake or the Albatross now easily completes in four or five hours, took an entire day of the Challenger's time.

The collections, when finally assembled at Sheerness, after the return of the ship, were contained in 2,270 jars, 1,749 bottles, 1,860 glass tubes, and 176 tin cases of alcoholics, with 22 casks of specimens in brine, and 180 tin cases of dried specimens, besides large quantities of material already sent home from Bermuda, Halifax, Capetown, Sydney, Hong Kong, and Japan.

The Challenger long ago resumed her barbaric function as an engine of war. Her trawls and dredges, battered and torn, hang upon the stair-rails in the Museum of naval architecture in South Kensington. Their share in the work

is done, but the collections are only now beginning to yield up the treasures of fact which they contain. The first of the final reports appeared in 1880; and now ten massive quarto volumes, crowded with sumptuous lithographed plates, have been printed, eight of these in the natural history series, two in the 'narrative,' which includes also the results of the physical observations. The completion of the entire series is promised for 1887, but it can safely be predicted that the last of the row of twenty volumes will not be placed upon our bookshelves before 1890. Preliminary reports have appeared to the number of at least three hundred: and, since it has been decided that the biological section of the British association is to devote its attention at the Montreal meeting almost exclusively to pelagic life, we may expect a large addition to the Challenger bibliography during the present year.



THE CHALLENGER.

The Challenger expedition was planned and executed solely in the interest of pure science, no utilitarian aims having ever been considered in its organization: it was the direct outgrowth of the previous expeditions of the Lightning and the Porcupine, inspired and conducted by Carpenter, Gwyn Jeffreys, and Wyville Thomson. The action of the British admiralty had, in consequence, a particularly salutary effect upon the policy of other nations; for it is highly probable, that, had there been no Challenger, the subsequent important explorations by Norway, Sweden, and Germany, and the expeditions of the Italian Washington and Violante, the French Travailleur and Talisman, the Dutch Willem Barents, and the American Blake, Fish Hawk, and Albatross, would not all have been carried on by grants from public treasuries. What the several governments might have done in fitting out ships, it is impossible for us to know. No one can question, however, that naturalists in all countries have been inspired and stimulated in a most salutary way by the action of the British government in publishing every half-year one of these sumptuously printed Challenger volumes, — each a collection of monographs from the hands of masterworkmen in natural history, not English only, but American, Scandinavian, Dutch, French, and Italian.

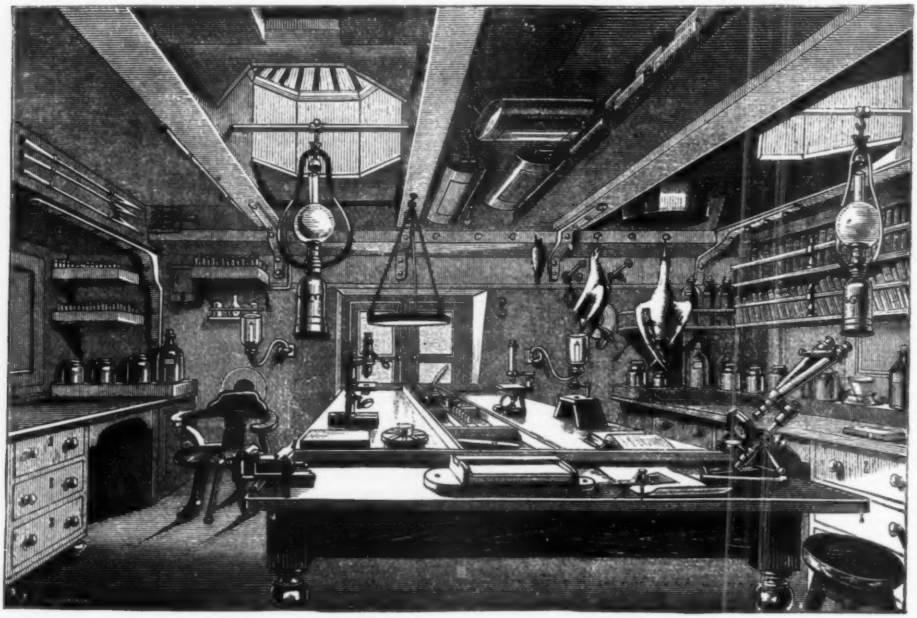
The history of the expedition, and the general nature of its discoveries, were long ago published to the world through Sir Wyville Thomson's 'The Atlantic,'[1] Professor Moseley's 'Notes,'[2] a work which should stand always on the same shelf with Darwin's 'Voyage of a naturalist,' Lord George Campbell's 'Log letters from the Challenger' (London, 1876),

[1] The voyage of the Challenger, — the Atlantic; a preliminary account of the exploring voyage of H. M. S. Challenger. 1878. 2 vols. 8°.

[2] Notes by a naturalist on the Challenger. London, 1879. 620 p. 8°.

Engineer Spry's illustrated journal in folio, with its hundreds of graphic sketches of scenery and incident,[1] and Mr. J. J. Wild's suggestive little books, 'Thalassa' and 'At anchor.'

The first-mentioned work, being semi-official in character, has been made the subject of much criticism, on account of the loose and inaccurate way in which many of the discoveries are announced. It is, in fact, a reprint of a series of letters to *Good words*, a family magazine, which were written by the director during the latter part of the voyage to satisfy public curiosity as to what had been done during its beginning. While it is undoubtedly open to criticism, it is probably as scholarly a piece of work as most landsmen would have been able to accomplish in the midst of the depressing influences of ship-life; and it is so much more satisfactory than any other official attempts at narratives which have yet appeared in connection with similar expeditions, that one cannot help regretting that the Pacific was not written up by the same hand and in the same manner. Sir Wyville Thomson has also been severely criticised for his policy in withholding the collections from the British museum, establishing the office of the expedition in Edinburgh, and refusing to ask the direct co-operation of the authorities of the British museum in working up his results. It is quite probable, to be sure, that a certain amount of additional support might have been gained by pursuing a different policy, but it is difficult to imagine whence it would have come. The British museum, like our own National museum, is the legal and proper place of deposit for government collections which have been worked up and reported upon, and the Challenger collections are gradually being sent there. The director of the expedition was, however, better fitted, both through experience and interest, to administer upon the collections brought together by his staff, than the officers of the natural history section of the British museum, no matter how much they may have excelled him as masters of special branches of work. Then, too, these men were already so overburdened with official routine that they could not have given the prominence to the Challenger work which it for the time de-



NATURAL HISTORY WORKROOM ON BOARD THE CHALLENGER.

[1] The cruise of her Majesty's ship Challenger. London, 1876.

served. The policy laid down by Professor Thomson, when called upon by the admiralty to propose a plan for the disposal of the Challenger collections, was in principle exactly consistent with that for many years pursued by our government geological survey, fish-commission, and bureau of ethnology, in relation to the national museum, though the heads of these organizations generally find it more convenient to use the organization of the museum to facilitate the administration of their own work.

A much more serious complaint has been based upon the policy of the director in claiming a right to control the results of the studies of his assistants during the voyage, and to announce their discoveries in his official capacity, without giving credit to the observers. It is, of course, impossible to say to what extent this policy was put in practice; but it is certain that the efficiency of the staff was to some extent impaired by it, and that some of the men felt called upon to protect themselves by writing their journals in languages unknown to the director. The subject has, of course, had no public discussion in England, and is referred to in this review simply on account of the general principle involved, which has already affected the efficiency of many institutions and expeditions in the United States. For the benefit both of science and of the workers in science, it is exceedingly important that there should be established some exact understanding of what constitutes literary or scientific property, and how much control over the results of the labors of his pupils or assistants a teacher or director justly may exert.

Whatever may have been the obstacles to the success of the expedition, its final results cannot fail to be satisfactory to every one who examines them. The highest praise is due to the late Sir Wyville Thomson, by whom it was organized and so successfully carried on. The liberal spirit with which he invoked the coöperation of foreign specialists was one of the many noteworthy features of his administration. Since his death, in March, 1882, the administration has been admirably carried on by Mr. John Murray, who was Professor Thomson's first assistant in natural history from the very start.

As has been already stated, eight volumes of zoölogical reports have already appeared. These contain the zoölogical monographs up to No. xxiv., thirty more still remaining to be published, together with two botanical reports, several concluding parts of papers already begun, and Mr. Murray's final summary of results. In discussing the publications of the expedition, the monographs will be taken up in systematic sequence. Their present order is arbitrary and temporary, it being understood that this will be abandoned in the final arrangement and combinations of the volumes of the report.

The mammal collections were assigned to Professor William Turner of the University of Edinburgh, whose paper upon the human crania is announced to be nearly ready, but whose final report on the marine mammalia will, it is feared, be long delayed. An instalment of the latter is, however, already in type, in the form of a report upon the bones of Cetacea (vol. i., 43 p., 3 pl.). This paper is a curious illustration of how many important facts may be derived from the study of a collection of objects of the most heterogeneous and miscellaneous character, such as the series of whale-bones gathered by such an expedition must necessarily be. The descriptions of the skeletons of Mesoplodon Layardi, and other whales obtained at the shore-stations, are valuable to the cetologist; but the greatest interest is in the account of the hundreds of separate bones dredged from the abyssal depths. At one station in the middle of the South Pacific, at 2,335 fathoms, there were obtained about ninety tympanic bullae, as well as numerous other ear-bones, the remains of nearly as many individual whales, most of them ziphioids. From the evidence of such fragments, Professor Turner concludes that the genus Mesoplodon is particularly abundant in the South Pacific, and Ziphius in the South Atlantic, though but few of these animals have been observed in those regions. Strange as it may seem, there were found no bones of the sperm-whale, so abundant in all the waters traversed by the ship. In the localities where bones were found, — none of which, it may be noted, were north of the equator, — the deposit at the bottom was a red clay, containing, besides the ear-bones, many hundreds of sharks' teeth, belonging to the genera Carcharodus, Oxyrhina, and Lamna, and apparently to extinct species. The question naturally arises, whether the associated cetacean remains belong to recent or extinct species. The occurrence of the teeth of tertiary sharks, lying so loosely upon the bottom that they may be scraped up by the dredge, indicates to the writer of this review that tertiary sharks have probably existed in these waters within comparatively recent times, and that the ear-bones, which cannot be referred to living species already known, in all likelihood belong to living species of whales not yet discovered. That interesting gener-

alized type of selachian from Japan recently announced by Garman under the name Chlamydoselachus is but one of the many signs that our knowledge of pelagic and abyssal life is still very incomplete.

Prof. D. J. Cunningham of the Royal college of surgeons, Ireland, contributes an essay upon the anatomy of certain marsupials and upon the mammalian *pes* (vol. v., 192 p., 13 pl.). The first part of this paper is descriptive, and devoted to Thylacine, Cuscus, and Phascogale; but its preparation led to a general investigation of the foot of mammals, involving the dissection of forty-five species distributed through all the orders. Professor Cunningham's conclusions as to the relations of the intrinsic muscles and nerves of the *pes* in different genera are of great interest, but, being merely incidental to the work of the Challenger, must be passed by with simple mention.

Vol. ii. is chiefly devoted to the report on birds, which is the eighth in the zoölogical series. This is a compound paper in thirteen parts, prepared by the standard British authorities, Sclater, Salvin, Saunders, Forbes, Tweeddale, and Garrod; one paper being also supplied by Salvadori of Turin, and one by Finsch of Bremen. The report on the anatomy of the petrels (Tubinares), by the late W. A. Forbes (vol. iv., 64 p., 7 pl.), is important as throwing much new light on the classification of these remarkable birds. It is based upon collections from the stores of the zoölogical society and the U. S. national museum as well as of the Challenger. The affinities of the petrels are shown to be with the Steganopoda and the storks and herons, rather than with the gulls. The most extensive anatomical monograph is that of the penguins, by Professor Morrison Watson of Owens college, Manchester, of which the first part has been printed (vol. vii., 244 p., 19 pl.). The publication of the second part will complete the ornithological work of the expedition. This essay is full of interest to the general reader as well as to the ornithotomist; since, although structural minutiae are fully discussed, each detail is brightened by some allusion to function, origin, or habit. The conclusions of Professor Watson, concerning the affinities of the Spheniscidae to each other and to other birds, are worthy of much fuller discussion. Many and appreciative allusions are made to Dr. Elliott Coues' monograph of the Spheniscidae, which is frequently quoted.

Professor W. Kitchen Parker's report on the development of the green turtle (vol. i., 58 p., 13 pl.) is an exceedingly weighty contribution to morphology, and concludes with a page of most suggestive generalizations upon the phylogeny of the Chelonia and Reptilia. This investigation was based upon a series of embryos obtained at Ascension Island, in compliance with Professor Parker's particular request, and is one of the most important of the side issues of the expedition.

Dr. Albert Günther's report on the shore fishes (vol. i., 82 p., 32 pl.) contains the identifications of fourteen hundred specimens, representing five hundred and twenty species, of which ninety-four were new. It consists of a series of faunal and regional lists, some of which, particularly those for remote oceanic islands, cover fields hitherto unexplored by ichthyologists; such as St. Paul's Rocks, Ascension, Kerguelen Island, and Juan Fernandez, and also Magellan Straits and the Arafura Sea. The systematic arrangement is all that can be desired: it is to be regretted, however, that the author has been satisfied to publish such brief and cursory diagnoses, and that he gives no tables of proportional measurements, thus causing serious embarrassment to students who have no access to his types. The report upon deep-sea fishes by the same author, at one time announced for vol. iii., is now so far down upon the official list of 'memoirs to follow in subsequent years,' that it is not likely to come to view for a long time. This is all the more to be regretted, since the fishes of the abyssal region are more peculiar, and more generally instructive, than perhaps the members of any other group. Much unstudied material in Italy, France, Austria, and America, is being held until these collections, now eight years in the author's hands, can be made known to the public. The preliminary descriptions published in 1878 are so meagre as to be nearly useless to any one except their author; and the type specimens themselves will, of course, be inaccessible for comparison until the final report is in type. Dr. Günther's success in re-organizing the natural history section of the British museum has been very great, yet it seems unfortunate that administrative work should so entirely monopolize the time of so eminent an ichthyologist.

RAIN IN BELGIUM.

La pluie en Belgique. Par A. LANCASTER. Bruxelles, *Hayez*, 1884. [Extract from the Annuaire de l'Observatoire royal de Bruxelles.] 113 p. 16°.

THE completion of a fifty-years series of uninterrupted observations of rainfall at Brussels

is taken by Lancaster as a fitting occasion for the preparation of a neat compendium of rain-records for all Belgium. The longer series, besides Brussels, are forty-three years at Ghent, thirty-five at Liége, twenty-three for Ostende, and twenty for Les Waleffes. The entire list, prepared for the end of 1882, contains one hundred and twenty-seven stations, with an average record of four and a half years; but of these, thirty-eight are for only one year, and sixty are for two years or less. At present the observatory receives the results attained at over two hundred stations. The chief general conclusions, which, unfortunately, are not shown either by map or diagram, are as follows: along the littoral lowlands an annual fall of about 650 millimetres, rising to a maximum in the highlands of the Ardennes (altitude about 400 metres) of from 900 to 1,100 millimetres. For 1882, rain and altitude of station are thus related: —

Below 10 m. . . .	825 mm.	200 to 400 m. . . .	1,220 mm.
10 to 100 "	875 "	400 to 700 " . . .	1,375 "
100 to 200 "	1,020 "		

According to seasons, the ratios are, winter, 100; spring, 95; summer, 129; autumn, 119. Along the coast the maximum is in autumn: in the interior, it is in summer. Heavy rains occur chiefly in the summer. In Brussels, since 1833, there have been sixty-nine records of 25 to 50 millimetres of rain in a day, thirty-four of these being in June, July, and August. A general increase in the annual rainfall is suspected since 1865, the evidence being as follows: —

Ghent . .	1838-64, 753 mm.	Ghent . .	1865-82, 981 mm.
Brussels . .	1833-64, 700 "	Brussels . .	1865-82, 778 "
Liége . . .	1847-64, 743 "	Liége . . .	1865-82, 796 "

The sun-spot cycle does not find strong confirmation from the records at Brussels.

Minimum.		Maximum.		Difference.
1833	646 mm.	1837	714 mm.	68 mm.
1843	736 "	1848	750 "	14 "
1856	670 "	1860	695 "	25 "
1867	682 "	1870	737 "	55 "
1878	818 "	1882 4 . . .	824 "	6 "

The little volume is chiefly valuable as bringing the older records up to time, and preparing for future work with the greatly increased number of stations of the past few years.

A NEW ASTRONOMICAL JOURNAL.

Bulletin astronomique, publié sous les auspices de l'Observatoire de Paris. Par M. F. TISSERAND. Tome i., No. i., Janvier. Paris, *Gauthier-Villars*, 1884. 64 p. 8°.

THE great number of new observatories in France now beginning active work has rendered a publication of this character a necessary adjunct of the labors of the astronomers of that country; and it will embrace two distinct parts, the first of which will contain the late observations made at the French observatories, ephemerides of planets and comets, and memoirs and notices relating to various topics in theoretical and practical astronomy. The second part is to be devoted to notices of current astronomical news, and a *résumé* of the chief periodical publications and of memoirs. This latter feature is a most fortunate one, and will make the *Bulletin* a desideratum in all observatories and scientific libraries. The special periodicals embraced in the *revue* of the January *Bulletin* are the *Monthly notices* of the Royal astronomical society, *The observatory*, the *Sidereal messenger*, the *American journal of science*, *Copernicus*, and the *Astronomische nachrichten*. The first part of the same number contains a brief paper by Tisserand, on the theory of the motion of the small planet Pallas, followed by observations of the satellites of Mars and Neptune by the Henrys, of the comet Pons-Brooks by Bigourdan and Perigaud, — all these at the Paris observatory, — and observations of the latter object by Trépied, at Alger; ephemerides of the small planets Mnemosyne, Diana, Alcmene, and Parthenope; and is concluded with the first part of a paper by Schulhof and Bossert, on the late return of the comet of 1812. Appended to the January *Bulletin*, under the head of *Variétés*, are, a paper on *les phénomènes crépusculaires*, by Radau; the comets and planets of 1883, by Bigourdan; and the new observatory of Rio de la Plata.

It would be a matter of the greatest interest to those engaged in new and original research, if a department relating to unpublished investigations could be added to the *Bulletin*. Brief notes in such a department, relating to work already in hand, its progress at various stages, and to projected research, the material for which may be in process of accumulation only, would be likely to lead to a more effective and happy state of co-operation among astronomers and observatories than now exists.

The *Bulletin astronomique* is published from the press of Gauthier-Villars, and is gotten up in the attractive style, and with that good typo-

graphic taste, which characterize the house of this *imprimeur-libraire;* and, being under the immediate charge of so eminent and able an astronomer as Tisserand, we venture to predict for the new journal an auspicious future. Tisserand will have as *collaborateurs* Callandreau and Bigourdan of the Paris observatory, and Radau; and the *Bulletin* is expected to be issued hereafter at the beginning of each month.

INTELLIGENCE FROM AMERICAN SCIENTIFIC STATIONS.

GOVERNMENT ORGANIZATIONS.

Geological survey.

Paleozoic paleontology. — During March the survey turned over to the National museum the collection of Devonian fossils from the Eureka district of Nevada. It embraces the Devonian fauna of central Nevada, described by Mr. Charles D. Walcott in the paleontology of the Eureka district, exclusive of the Actinozoa and Bryozoa. It contains 2,397 specimens, including 203 species, 89 of which are Brachiopoda, 44 Lamellibranchiata, 40 Gasteropoda, 11 Pteropoda, 9 Cephalopoda, 3 Crustacea, and 7 Poecilopoda.

During the last field-season, Mr. C. D. Walcott and his assistants spent some time in the study of the Potsdam localities in New York, and considerable collections were sent in to the office of the survey. He is now engaged in the preliminary preparation and study of material to illustrate the Cambrian fauna of the United States. It is very desirable that large quantities of material should be brought together from all the Cambrian groups; and the survey would be glad to receive collections, whether large or small, from all portions of the country. Care should be taken in packing, and a record kept. Correspondence has been begun with numerous collectors in Wisconsin, in order to obtain material from the Potsdam group.

Mesozoic paleontology. — Dr. C. A. White, in charge of this branch of paleontologic research, has lately prepared a bulletin on mesozoic fossils. It is illustrated with nine plates, and contains three papers devoted respectively to the mesozoic fossils of Alaska, Arkansas, and Texas. Hyatt's new genus, Enclimatoceras, is described and illustrated in this bulletin. The fourth annual report, which is just about being issued, contains a paper of sixty pages, with forty-nine plates of illustration, entitled "A review of the fossil Ostreidae of North America, and a comparison of the fossil with the living forms."

Dr. White is making preliminary studies of fossils from the Pacific coast, preparatory to visiting that section to undertake a special study of its mesozoic and cenozoic faunas.

At intervals ever since 1880, Dr. White has been engaged in the description of the mollusks and echinoderms of the cretaceous formation of the provinces of Sergiee, Pernambuco, Bahia, and Para, in Brazil. The collections were made by the Imperial geological survey of Brazil, in charge of Ch. Fred. Hartt. Dr. O. A. Derby, his former assistant, is in charge of the geological division of the National museum of Brazil, under the auspices of which this work is done. The specimens described include 82 species of Chonchifera, 91 of Gasteropoda, 13 Cephalopoda, 11 of fresh-water faunas, and 15 echinoderms. Among them he has established four new genera. The specimens are all referable to the Neocomian series, as is also a fresh-water fauna from Bahia, described in the same volume. The manuscript, with twenty-eight quarto plates of illustration to be lithographed, is now ready for the printer, and will be published in Portuguese in the *Archivos* of the Brazilian national museum. It is expected that the descriptions, at least, will also be published in English. Dr. White finds that this cretaceous fauna is quite unlike any in North America, but more like that of southern India. A portion of the specimens have been identified with some described by Stoliczka in the 'Paleontologia Indica.'

Mr. J. B. Marcou, Dr. White's assistant, has been busily engaged in sorting and arranging the type specimens described by Prof. F. B. Meek in the various reports of Hayden, King, and other government publications. Many of the types of Conrad and Whitfield are also in the collections that he is arranging.

Mr. L. C. Johnson, who is in the same division, is arranging the large collections of fossils made by him in the Gulf states last summer, and is preparing geological sections from his notes taken while collecting these fossils.

Vertebrate paleontology. — Prof. O. C. Marsh, who is in charge of this division, reports progress in the preparation of the various memoirs, and states that field-work which began in April starts under good auspices, careful preparations having been completed to place four parties in the field early in the season, to be followed by others later.

Paleo-botany. — Among the collections of fossil plants made by Prof. L. F. Ward from the Fort-Union group in the upper Missouri and Yellowstone region, a number of new specimens have been found, which will eventually be published in Professor Ward's memoir on the subject. Professor Ward at present is engaged on the introduction to this work, in which he proposes to review the subject of paleobotany from the historical, geological, and biological stand-point. The work of figuring the types from the Fort-Union collections has been commenced, and a card-catalogue has been made of all the species of fossil plants in the National museum, which renders it much more available as an aid to research than it has ever been before.

RECENT PROCEEDINGS OF SCIENTIFIC SOCIETIES.

Chemical society, Washington.

April 24. — Thomas Taylor presented a paper upon oleomargarine, butter, and butterine, in which he described methods for the recognition of imitation-butter by means of the microscope, polariscope, and sulphuric-acid test. Under the microscope, oleomargarine shows marked crystallization, pure butter is perfectly even, and butterine contains excess of oily matter. With sulphuric acid, butterine gives a yellowish tint, which passes through a brownish amber to a final vandyke-brown color. Oleomargarine gives a yellow color, which gradually deepens to a red or crimson. With butter, sulphuric acid produces a whitish tinge at first, which soon changes to a salmon color, but never becomes crimson. If the butter has been colored with annatto, the color will be bluish green to black at first, but brick-red or salmon afterwards. With the polariscope and selenite plate, butterine gives a uniform plain tinge, red or green, while oleomargarine is distinguished by the colors produced by fat crystals.—— Dr. T. M. Chatard gave an account of a modified process for the estimation of alkalies in silicates, which is based upon Hempel's method of decomposition with bismuth oxide. One part of the pulverized mineral, intimately mixed with two parts of bismuth oxide, is heated to redness over a Bunsen burner for about twenty minutes. The mass is then decomposed by strong hydrochloric acid; the excess of acid is evaporated off; the diluted solution is precipitated by ammonia and ammonia carbonate, and filtered. In the filtrate are the alkalies and magnesia, to be separated by the usual method. The process is easy and rapid. —— Mr. E. Richards next gave some notes on the specific gravity of milk and whey. The whey seems to be more constant in specific gravity than milk. —— A. E. Knorr and H. W. Wiley described the manufacture and use of very thin glass dishes for certain purposes in the analysis of organic products. The dishes are so thin and light that they may be pulverized, with their contents, preliminary to the combination analysis of the latter. Several of them were made before the society.

Natural science association, Staten Island.

April 12. — Mr. Charles Butler read a paper on experiments upon cross-breeding of moths. Last July he placed a recently hatched female of Callosamia Promethea out-doors in a box to secure some males of the same species, but caught six male C. angulifera within an hour of the time of the first exposure. The following night he caught five more with a new female, and the next night only one, and no C. Promethea. He noticed the fact that both of these species are not found about the vivarium at the same time. C. Promethea comes in the afternoon, when the sun is still up: C. angulifera comes after sunset. Mr. Bruice of Brockport says that a female Samia ceanothi, a California species hatched by him, attracted the males of S. Cecropia in great numbers. Mr. Pilate of Dayton, O., states that he tied a female S. Cynthia out-doors over night, in order to obtain some eggs, and the next morning found her attached to a male S. Cecropia. Mr. Cook of Lansing, Mich., had a female S. Cecropia that attracted a score or more of S. Columbia.

Society of arts, Massachusetts institute of technology.

April 10. — Mr. Thomas Pray, jun., spoke on the subject of the cotton-fibre and its structure, as shown by the microscope. A photograph was shown of a stalk carrying two hundred and twenty-five ripened bolls: and it was stated, that by the method lately practised, of developing this variety by carefully selecting the seeds of the best bolls, and planting them apart from others, and continuing this process during several years, it had been conclusively proved that it was perfectly possible to bring the yield of lowland cotton up to three bales per acre, with proper fertilization and favorable season; whereas, at present, a yield of one bale per acre is very large, the average being less than one bale to two acres. In the method of ginning at present practised, the seeds of all the different varieties of cotton ginned are indiscriminately mixed; and the planter who brings his cotton to the gin, though receiving the same cotton back again, gets, for every bale ginned, a certain number of bushels of this miscellaneous mixture of different seeds, from which his next year's crop is planted. No pains whatever have been taken to develop good varieties of cotton, or to increase the yield per acre by a process of selection, except in rare cases within a few years. A number of photo-micrographs of cotton-fibres were shown, exhibiting their peculiarities of structure. The fibre is a wide or flat cylinder, with a thickening at the edges, and thin in the centre, twisted into a spiral. The coloring-matter is oily or resinous, and, in drying, is deposited on the edges of little pockets occurring along the fibre. The better the fibre, the more perfect its spirality, and the more regular in shape and in position are these oily deposits. Wrinkles are also formed in the fibre when drying, becoming most marked in the perfectly matured dry fibre, thus affording another test of good cotton. The lack of spirality prevents the fibre from being twisted with others in such a way as to become intimately engaged with them. An illustration was shown of the cotton after it had passed through the saw-gin, clearly showing that the fibres had been torn apart, the ends showing plainly the mutilation by the saw-teeth. The speaker referred to the faults of the present methods of ginning, and pointed out the directions for improvement. An illustration was also shown of cotton, after passing the railway head, which is supposed to make the fibres parallel; whereas such is by no means the case. The speaker urged the importance of examining cotton by the microscope, and dwelt upon the advantages which manufacturing corporations would gain by selecting their stock in this way.

NOTES AND NEWS.

THE following is the list of subscriptions sent to the editor of *Science* for a memorial tablet inscribed with the name of JOACHIM BARRANDE, to be placed on the side of a cliff at Kuchelbad, near Prague, Bohemia: Professor Alpheus Hyatt, Boston, \$10; Dr. Carl Rominger, Ann Arbor, \$10; John Collett, state geologist, Indianapolis, \$2; Professor Jules Marcou, Cambridge, \$4; S. H. Scudder, Cambridge, \$10; and the following from gentlemen connected with the U. S. geological survey: Major J. W. Powell, \$5; C. D. Walcott, \$5; Dr. C. A. White, \$5; Dr. A. C. Peale, \$1; G. K. Gilbert, \$3; Capt. C. E. Dutton, \$1; Prof. T. C. Chamberlin, \$3; W. J. McGee, \$2; J. B. Marcou, \$2; Prof. H. S. Williams, \$5; Prof. S. G. Williams, \$2; S. F. Emmons, \$2: total, \$72. A draft for 175.60 Austrian florins was forwarded, on Monday last, to Dr. Anton Fritsch of the Prag museum.

— The first number of the *Bulletin of Massachusetts natural history* has appeared. The contents, unfortunately, show no justification for its existence, either for the interests of the natural history of Massachusetts, or as a record of work in the State agricultural college, which its circular declared to be its two special objects.

— The *Kölnische zeitung* of April 10 states that Dr. Koch, the head of the German scientific commission for the investigation of the cholera, has submitted a seventh report, dated Calcutta, March 4. It mentions the important discovery that the storage-basins called tanks have proved the locally limited seat of cholera infection and communication. Little ponds or swamps, scattered over all Bengal in large numbers, surrounded by cottages, furnish to the dwellers near them their water-supply, and are used for various purposes, — as for bathing, clothes-washing, for cleaning domestic utensils, and also for drinking-water. After the commission had in vain sought for the cholera bacillus in numerous trials of tank, sewage, and river water, they are discovered for the first time in a tank in the midst of the cholera district. Since the last report, the bodies of twenty cholera victims, and the excrements of eleven patients, have been examined. The whole number of cases examined in India now amounts to forty-two dead bodies and twenty-eight patients. The last cases have not, to be sure, yielded new results. They resemble the others in every particular, especially in reference to the behavior of the bacilli. In addition, there are still in progress investigations concerning the influence of various substances — as sublimate, carbolic acid, and other disinfectants — on the development of the cholera bacilli in culture-fluids; also concerning their behavior in carbonic acid, and deprived of air. Attempts to discover a lasting form of the bacillus were also continually made. Up to the present time, nothing of the kind has been discovered. The only possibility of getting bacilli capable of living a longer time is to keep them from drying. In liquids they remain for weeks capable of development, and every thing seems to indicate that only in a moist condition can they be preserved, and then made to act on human bodies. Unfortunately, on account of the warm weather, which this year begins early, further investigations on this subject must be abandoned. Dr. Koch is returning to Europe.

— The first general meeting of the British association at Montreal will be held on Wednesday, Aug. 27, at 8 P.M. precisely, when Professor Cayley will resign the chair, and Professor the Right Hon. Lord Rayleigh, president-elect, will assume the presidency, and deliver an address. On Thursday evening, Aug. 28, at 8 P.M., there will be given a *soirée;* on Friday evening, Aug. 29, at 8.30 P.M., a discourse, by Prof. W. G. Adams; on Monday evening, Sept. 1, at 8.30 P.M., a discourse on 'The modern microscope in researches on the least and lowest forms of life,' by the Rev. W. H. Dallinger; on Tuesday evening, Sept. 2, at 8 P.M., a *soirée;* on Wednesday, Sept. 3, the concluding general meeting will be held at 2.30 P.M.; Saturday evening, Aug. 30, a lecture on 'Comets,' by Prof. R. S. Ball, of Dublin university, and astronomer royal for Ireland (this does not appear in the association circular, as it is intended for the citizens of Montreal).

— An informal meeting was held April 12, in Philadelphia, to discuss the plans of the proposed department of biology at the University of Pennsylvania. A fund of a hundred thousand dollars is to be raised to allow of an expenditure of fifteen thousand dollars for a building, the balance to be invested for the support of the institution. Twenty thousand dollars have already been subscribed, and the contracts for the building will soon be given out. A committee consisting of Hon. John Welsh, Dr. Leidy, Dr. Alice Bennett, Mrs. S. A. Crozer, Dr. Horace F. Jayne, Miss Ida Wood, and Miss Mary Thorn Lewis, was appointed to carry out the purposes of the meeting.

— At the meeting of the French academy on March 31, Mr. Charles Brongniart announced the discovery, in the rich carboniferous deposits of Commentry, of new gigantic forms of insects, of the type of Dictyoneura, in which the expanse of wings was nearly twenty-eight inches.

— Among recent deaths, we note that of Abbé Brugnone, the paleontologist, at Palermo, on the 3d of February, in his seventieth year; that of Dr. E. Behm, editor-in-chief of Petermann's *Geographische mittheilungen*, already noticed, occurred at Gotha, on the 15th of March; Capt. Niels Hoffmeyer, the director of the meteorological institute at Copenhagen, died at that place, the 16th of February.

— It is proposed to collect as complete a set of books on electricity as possible, to be shown at the International electrical exhibition in Philadelphia this autumn. After the exhibition the collection will be placed in charge of the Franklin institute, and will form the nucleus of a reference-library.

— E. and F. N. Spon announce as in preparation 'The electric light,' by E. Alglave and J. Boulard, translated from the French by T. O'Connor Sloane.